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PROCEEDINGS

OF THE

PHYSICAL SOCIETY OF LONDON.

From December 1910 *to August* 1911.

VOL. XXIII.

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CONTENTS.

VOL. XXIII.

PAGE

Proceedings at the Meetings of the Physical Society of London, Session 1910-1911 .... vii.

The Radio-Balance. A Thermoelectric Balance for the Absolute Measurement of Radiation, with Applications to Radium and its Emanation. By H. L. CALLENDAR, M.A., LL.D., F.R.S. 1

Demonstration of a New Method for Producing High-tension Discharges. By Prof. ERNEST WILSON and Mr. W. H. WILSON 35

The Behaviour of Steel under Combined Static Stress and Shock. By F. ROGERS 39

On the Supposed Propagation of "Equatorial" Magnetic Disturbances with Velocities of the order of 100 miles per second. By C. CHREE, Sc.D., LL.D., F.R.S. 49

On Cusped Waves of Light and the Theory of the Rainbow. By W. B. MORTON, M.A. 58

The Afterglow of Electric Discharge. By the Hon. R. J. STRUTT, F.R.S. 66

The Approximate Solution of Various Boundary Problems by Surface Integration combined with Freehand Graphs. By L. F. RICHARDSON 75

The Electric Stress at which Ionisation Begins in Air. By ALEXANDER RUSSELL, M.A., D.Sc., M.I.E.E. 86

PAGE

A Demonstration of the Phase Difference between the Primary and Secondary Currents of a Transformer by Means of Simple Apparatus. By F. T. Trouton, Sc.D., F.R.S., Professor of Physics, University College, London... 98

A Note on the Experimental Measurement of the High-Frequency Resistance of Wires. By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London 103

The Measurement of Energy Losses in Condensers Traversed by High-Frequency Electric Oscillations. By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and G. B. Dyke, B.Sc., A.M.Inst.E.E. 117

Some Resonance Curves taken with Impact and Spark Ball-Dischargers. By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and G. B. Dyke, B.Sc., A.M.Inst.E.E. 136

On Flames of Low Temperature Supported by Ozone. By the Hon. R. J. Strutt, F.R.S., Professor of Physics, Imperial College of Science, S. Kensington........ 147

The Caloric Theory of Heat and Carnot's Principle. By H. L. Callendar, M.A., LL.D., F.R.S., Professor of Physics at the Imperial College of Science and Technology, S.W. 153

On the Movement of a Coloured Index along a Capillary Tube, and its Appliaction to the Measurement of the Circulation of Water in a Closed Circuit. By Albert Griffiths, D.Sc., Head of Physics Department, Birkbeck College 190

On an Optical Lever of High Power suitable for the Determination of Small Thicknesses and Displacements. By E. H. Rayner 198

A Demonstration of the Working of the Anschütz Gyro Compass. By G. K. B. Elphinstone........ 201

Note on an Electrical Trevelyan Rocker. By W. H. Eccles, A.R.C.S., D.Sc........ 204

PAGE

Some Notes on the Tilted Gold Leaf Electrometer, with Suggestions as to the Manipulation of Gold Leaf Suspensions, Suitable Insulators for Electrometer Work, &c., By G. W. C. Kaye, D.Sc., B.A., The Natonal Physical Laboratory 209

The Roots of the Neumann and Bessel Functions. By John R. Airey, M.A., B.Sc., late Scholar of St. John's College, Cambridge 219

The Vibrations of Circular Plates and their Relation to Bessel Functions. By John R. Airey, M.A., B.Sc., late Scholar of St. John's College, Cambridge 225

On the Curvature of the Spectrum Lines in a Concave Grating. By Albert Eagle, B.Sc., A.R.C.S., Imperial College of Science and Technology 233

Oscillatory Currents in Coupled Circuits. By G. W. O. Howe, M.Sc., Whit. Sch. 237

High-tension Electrostatic Wattmeters. By Ernest Wilson 246

Previous Magnetic History as Affected by Temperature. By Prof. E. Wilson and L. C. Budd 253

Notes on the Behaviour of Incandescent Lime Kathodes. By R. S. Willows, M.A., D.Sc., and T. Picton, M.A., B.Sc. 257

On the Formation of Dust Striations by an Electric Spark. By S. Marsh, B.Sc., Ph.D., late Fellow of University of Wales, Lecturer in Physics at Battersea Polytechnic; and W. H. Nottage, B.Sc., Demonstrator in Physics at Battersea Polytechnic 264

The Method of Constant Rate of Change of Flux as a Standard for Determining Magnetisation Curves of Iron. By J. T. Morris and T. H. Langford, B.Sc. 277

Some Radio-Telegraphic Apparatus in Use at the City and Guilds Engineering College. By G. W. O. Howe, M.Sc. 301

The Lüders' Lines on Mild Steel. By W. Mason, M.Sc., Liverpool University 305

PAGE

A New Method of Approximate Harmonic Analysis by Selected Ordinates. By Prof. S. P. Thompson, F.R.S. 334

Short Table of Circular and Hyperbolic Functions for Complex Values of the Argument. By A. Johnstone 344

On the Measurement of Contact Differences of Potential. By Prof. A. Anderson and J. E. Bowen 346

The Capacity Coefficients of Spherical Electrodes. By Dr. A. Russell... 352

On the Effect of a Narrow Saw-cut in the Edge of a Conducting Strip on the Potential and Stream Lines in the Strip and on the Resistance of the Strip. By Charles H. Lees, D.Sc., F.R.S., Professor of Physics in the East London College, University of London 361

The Probability Distribution of the Time Intervals of α Particles with Application to the Number of α Particles emitted by Uranium. By E. Marsden, M.Sc., and T. Barratt, B.Sc., East London College, University of London 367

On the Benkö Primary Battery. By W. R. Cooper, M.A.. 374

Index 377

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1910-1911.

October 28th, 1910.

Meeting held at King's College by kind invitation of Prof. E. Wilson.

Prof. H. L. Callendar, F.R.S., President, in the Chair.

The following Papers were read :—

1. Demonstration of a New Method for Producing High-tension Discharges. By Prof. Ernest Wilson and Mr. W. H. Wilson.
2. The Behaviour of Steel under Combined Static Stress and Shock. By Mr. F. Rogers.

November 11th, 1910.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. Callendar, F.R.S., President, in the Chair.

The following Papers were read :—

1. On the Supposed Propagation of "Equational" Magnetic Disturbances with Velocities of the Order of 100 miles per second. By Dr. C. Chree, F.R.S.
2. On Cusped Waves of Light and the Theory of the Rainbow. By Prof. W. B. Morton.

November 25th, 1910.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. CALLENDAR, F.R.S., President, in the Chair.

The following Papers were read :—

1. The Electric Stress at which Ionisation begins in Air. By Dr. A. RUSSELL.

2. The Afterglow of the Electric Discharge. By Prof. R. J. STRUTT, F.R.S.

A surface brightness photometer was exhibited by Mr. J. S. Dow.

A Paper on The Approximate Solution of Various Boundary Problems by surface integration combined with freehand graphs, by Mr. L. F. RICHARDSON, was taken as read.

December 20th, 1910.

An Exhibition of Apparatus was held at the Imperial College of Science and Technology, the following firms taking part :—

R. & J. Beck ; British Radio-Telegraph and Telephone Co., Ltd. ; Cambridge Scientific Instrument Co. ; Casella & Co. ; A. C. Cossor Ltd. ; H. W. Cox & Co. ; J. H. Dallmeyer Ltd. ; Elliott Bros. ; Everett, Edgcumbe & Co. ; Evershed & Vignoles Ltd. ; Foster Instrument Co. ; Gallenkamp & Co. : Gambrell Bros. ; J. J. Griffin & Sons ; J. J. Hicks & Co. ; A. Hilger Ltd. ; India Rubber, Gutta Percha, & Telegraph Works, Ltd. ; Isenthal & Co. ; E. Leitz ; Marconi's Wireless Telegraph Co. ; Leslie Miller ; Morris & Lister Ltd. ; Muirhead & Co. ; Negretti & Zambra ; Newton & Co. ; R. W. Paul ; W. G. Pye & Co. ; Reynolds & Bransom ; Strange & Graham Ltd. ; H. Tinsley & Co. ; Townson & Mercer ; Weston Electrical Instrument Co. ; Wright & Co. ; Carl Zeiss, Ltd.

The following short discourses were given :—

On some Improvements in Transmitters and Receivers for Wireless Telegraphy. By Prof. J. A. FLEMING, M.A., D.Sc., F.R.S.

Short Cinematograph Demonstrations of Moving Diagrams of Lines of Force, by Prof. S. P. THOMPSON, F.R.S., and of Sound-wave Forms, by Prof. R. W. WOOD, were exhibited by Mr. R. W. PAUL.

January 27th, 1911.

Meeting held at University College, Gower Street, by kind invitation of Prof. F. T. **Trouton**.

Prof. C. H. **Lees**, F.R.S., Vice-President, in the Chair.

The following Papers were read :—

1. A Demonstration of the Phase Difference between the Primary and Secondary Currents of a Transformer by means of Simple Apparatus. By Prof. F. T. **Trouton**, F.R.S.
2. A Note on the Experimental Measurement of the High Frequency Resistance of Wires. By Prof. J. A. **Fleming**, F.R.S
3. The Measurements of Energy Losses in Condensers Traversed by High Frequency Electric Oscillations. By Prof. J. A. **Fleming** and Mr. G. B. **Dyke**.
4. Some Resonance Curves taken with Impact and Spark Ball Dischargers. By Prof. J. A. **Fleming** and Mr. G. B. **Dyke**.

Annual General Meeting.

February 10th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

The following Report of the Council was read by the Secretary :

Since the last Annual General Meeting there have been held eleven ordinary Meetings and two informal Meetings. Of these, ten were held at the Imperial College of Science, one at King's College, one at University College, and one at the National Physical Laboratory.

The average attendance, excluding the Exhibition and the informal Meeting at the National Physical Laboratory, was 55, as compared with 41 last session.

The sixth Annual Exhibition of Apparatus by Manufacturers was held on December the 20th in the afternoon and evening and attracted a larger attendance than in any previous year, the number of Fellows and Visitors being about 800. An experimental discourse on " Some Improvements in Transmitters and Receivers for Wireless Telegraphy " was kindly given by Prof. J. A. Fleming, D.Sc., F.R.S., at both Sessions, and Mr. R. W. Paul gave demonstrations of some magnetic and optical phenomena by means of cinematograph films. The number of exhibitors was 35.

The past session has been marked by an important change in the publications. It has been felt by the Council for some time that the delay experienced in publishing the " Proceedings " hitherto has been harmful to the Society, and they have therefore decided henceforth to issue the "Proceedings" in five parts, at two-monthly intervals, during each Session, and they hope it may be possible later on to issue ten parts at monthly intervals.

This change has involved the termination of the arrangement which has existed hitherto with Messrs. Taylor & Francis, whereby Papers selected by the Council have been offered to the Editors of the "Philosophical Magazine," and have in most cases been published in that journal. Papers now read before the Society, generally speaking, only appear in the Society's "Proceedings"; but, after reading, Fellows are at liberty to offer their Papers to any scientific publication, and revised proofs are in any case sent by the Council to a selected number of journals in the hope of promoting wider publication.

The present arrangement for a two-monthly issue enables the Society to publish accepted Papers very promptly. The first part of the current volume was issued last month after some delay due to the change in arrangements, and the second part will be issued in the next few days.

The Council hope that the improvement in the rapidity of publication will lead to an increase in the membership of the Society. Although the new scheme does not involve more expense, it is important that the membership should be increased as much as possible in order to provide the funds necessary to enable the Council to widen the scope and increase the activity of the Society. The Council have appointed Mr. A. Eagle as Assistant Secretary.

The number of Ordinary Fellows now on the roll, as distinct from Honorary Fellows, is 433, a decrease of 3 on the number last year; 12 new Fellows have been elected and one Fellow who resigned a few years ago has been reinstated. Three Honorary Fellows were elected at the last Annual General Meeting, namely, Prof. S. Arrhenius, Madame S. Curie and Prof. G. E. Hale. There have been 7 resignations, and 9 Fellows have been struck off the Register either for non-payment of subscriptions or because they cannot be traced. The Society has to mourn the loss by death of Major P. Cardew, Mr. H. W. Eve and Sir William Huggins, K.C.B.

The Report of the Council was adopted.

The Report of the Treasurer was read by the Treasurer.

The total income of the Society for the year 1910 is £19. 13s. 3d. less than the previous year. The difference is mainly due to a falling-off in the subscriptions, and this can be accounted for by arrears, which amounted at the end of the year to £44. 2s., as against £20 in the last accounts. If allowance is made for this difference it will be seen that the subscriptions to the Society are nearly stationary instead of increasing as they should do. A favourable point in the Society's balance-sheet is the increase in the revenue from the sales of publications. It is to be hoped that the new arrangement for more prompt publication of the Proceedings will still further increase this item.

Turning to the expenditure side of the accounts the sum spent upon the publications of the Society is still unduly high. This is mainly due to the commendable efforts of our Secretary to catch up the arrears which had accumulated in past years. The arrears are completely worked off as far as the Proceedings of the Society are concerned, but some of the expenditure in connection therewith will come into the accounts for the current year, and consequently make them above the normal. The total expenditure of the Society shows an increase of £21. 5s. 3d. over the previous year. The net result of the year is a deficit of £18. 15s. 5d. This unsatisfactory result again emphasizes the necessity of an increase in the membership of the Society so as to give a margin of income over expenditure. The fact that the accounts show a deficit for the year 1910 need not alarm the Fellows of the Society, as it is mainly caused by the large sums the Society has been spending on its printing account.

The present accounts show for the first time a separate statement of the life compositions received by the Society, as the Council has decided that during the life of any Fellow who has compounded for his subscription all sums paid by him as composition shall be invested, and the interest thereon only shall be applied to the current expenditure of the Society, and after his death the amount of the composition shall be transferred to the general funds of the Society.

In order to start the fund the compositions paid by each of the life Fellows of the Society on the books at the 31st December last have been included in a separate account, the total of which amounts to £2, 209.10s.

In future all composition fees will be credited to this account, and on the death of a life Fellow his composition will be transferred to the current account.

The property account of the Society shows but little change. What change there is is caused by the above-mentioned deficit on current account, and by market fluctuations of the securities in which the Society's funds are invested. The figures appearing in the account are the value of the securities at the market prices on December 31st, 1910, kindly supplied by the Manager of Parr's Bank.

Following the custom of previous years the publications in stock appear in the property account at £200. They have not been revalued. During the current year the publications in stock will be revalued, and the new valuation will appear in the current year's accounts.

The Report of the Treasurer and the Balance Sheet, which was also presented to the Meeting, were adopted.

The Treasurer in Account with the Physical Society, from January, 1910, to December 31st, 1910.

Dr.	£	*s.*	*d.*	£	*s.*	*d.*
Entrance Fees	16	16	0			
Subscriptions	457	16	0			
Extra Subscriptions	28	5	0			
Arrears	42	0	0			
Abstracts, &c.	10	16	0			
Students	3	13	6			
				559	6	6
Composition Fees				21	0	0
Dividends:—						
Furness Debenture Stock	15	1	2			
Midland Railway	37	13	4			
Metropolitan Board of Works	6	12	0			
Lancaster Corporation Stock	11	6	0			
New South Wales	8	7	6			
London, Brighton & South Coast Railway	24	2	7			
Great Eastern Railway	18	16	8			
				121	19	3
Sales of Publications				55	1	11
One Sale of Royal Society Proceedings				1	1	0
Income Tax recovered				26	12	8
Interest on £400 deposit account				8	17	10
				793	19	2
January 1, 1910, balance at Bank				78	9	7
				£872	8	9

Cr.	£	*s.*	*d.*	£	*s.*	*d.*
Science Abstracts				277	10	0
Taylor & Francis:—						
Proceedings	250	12	9			
Abstracts, &c.	104	13	9			
Distribution	32	0	6			
Separate Copies	36	18	6			
				424	5	6
Periodicals				6	6	3
Binding				2	8	6
Reporting				35	0	0
Attendance				13	12	6
Refreshments and sundry expenses at meetings				12	18	4
Petty cash				19	10	6
Bank commission and stamped cheques				0	6	7
Subscriptions paid in error				2	7	0
Royal Astronomical Society				2	2	0
Royal Society				1	1	0
Deficit on Dinner Account				3	14	1
International Catalogue (2 years)				11	12	4
				812	14	7
Balance at Bank	164	8	0			
Less two cheques	104	13	10			
	59	14	2	59	14	2
				£872	8	9

WILLIAM DUDDELL, *Honorary Treasurer.*

Audited and found correct,
C. W. S. CRAWLEY.
A. WRIGHT.

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY, December 31st, 1910.

Assets.	£	s.	d.
Subscriptions due, Treasurer's estimate...	44	2	0
£533 Furness Ry. Co.3 per cent. Debenture Stock	405	0	0
£1,600 Midland Railway 2½ per cent. Preference Stock	1,064	0	0
£200 Metropolitan Board of Works 3½ per cent. Consolidated Stock	200	0	0
£400 Lancaster Corporation 3 per cent. Redeemable Stock	324	0	0
£284. 2s. 9d. New South Wales 3½ per cent. Inscribed Stock	246	0	0
£500 London, Brighton & South Coast Railway Ordinary Stock	565	0	0
£500 Great Eastern Railway 4 per cent. Debenture Stock	525	0	0
Balance at Bank	164	8	0
Ditto on deposit	400	0	0
Publications in stock	200	0	0
	£4,137	10	0

Liabilities.	£	s.	d.
Two cheques not presented	104	13	10
Life Composition Fund	2,209	10	0
Balance	1,823	6	2
	£4,137	10	0

WILLIAM DUDDELL, *Honorary Treasurer.*

Audited and found correct,

C. W. S. CRAWLEY.
A. WRIGHT.

LIFE COMPOSITION FUND.

		£	s.	d.
186	Fellows paid £10	1,860	0	0
3	Fellows paid £15	45	0	0
4	Fellows paid £21	84	0	0
7	Fellows paid £31. 10s.	220	10	0
200		£2,209	10	0

Audited and found correct,

C. W. S. CRAWLEY.
A. WRIGHT.

WILLIAM DUDDELL, *Honorary Treasurer.*

The election of Officers and other Members of Council then took place, the new Council being constituted as follows :—

President.—Prof. H. L. CALLENDAR, M.A., F.R.S.

Vice-Presidents who have filled the Office of President.—Prof. G. C. FOSTER, F.R.S. ; Prof. W. G. ADAMS, M.A., F.R.S. ; Prof. R. B. CLIFTON, M.A., F.R.S. ; Prof. A. W. REINOLD, M.A., F.R.S. ; Prof. Sir ARTHUR W. RÜCKER, M.A., D.Sc., F.R.S. ; Sir W. DE W. ABNEY, R.E., K.C.B., D.C.L., F.R.S. ; Prin. Sir OLIVER J. LODGE, D.Sc., F.R.S. ; Prof. S. P. THOMPSON, D.Sc., F.R.S. ; R. T. GLAZEBROOK, D.Sc., F.R.S. ; Prof. J. H. POYNTING, M.A., F.R.S. ; Prof. J. PERRY, F.R.S. ; C. CHREE, Sc.D., F.R.S.

Vice-Presidents.—A. CAMPBELL, B.A. ; Prof. C. H. LEES, D.Sc., F.R.S. ; Prof. T. MATHER, F.R.S. ; S. SKINNER, M.A.

Secretaries.—W. R. COOPER, M.A. ; S. W. J. SMITH, M.A., D.Sc.

Foreign Secretary.—Prof. S. P. THOMPSON, D.Sc., F.R.S.

Treasurer.—W. DUDDELL, F.R.S.

Librarian.—S. W. J. SMITH, M.A., D.Sc.

Other Members of Council.—W. H. ECCLES, D.Sc. ; A. GRIFFITHS, D.Sc. ; Major W. A. J. O'MEARA, C.M.G. ; A. RUSSELL, M.A., D.Sc. ; W. N. SHAW, M.A., Sc.D., F.R.S. ; F. E. SMITH ; Prof. the Hon. R. J. STRUTT, F.R.S. ; W. E. SUMPNER, D.Sc. ; R. S. WHIPPLE ; R. S. WILLOWS, M.A., D.Sc.

Prof. J. H. VAN'T HOFF was elected an Honorary Fellow of the Society.

Votes of thanks were passed to the Auditors, to the Officers and Council, and to the Governors of the Imperial College of Science and Technology.

Prof. H. L. CALLENDAR then delivered the President's Address.

PRESIDENTIAL ADDRESS.

The Society has been exceptionally fortunate this year in losing so few of its Members by death. So far as I know, there are only three Members to commemorate. The most important part of their work lay outside the scope of the activities of this Society, and I feel that any attempt which I may make to do honour to their memory will of necessity be inadequate, and that their achievements will be more fittingly recorded elsewhere.

The great pioneer of stellar spectroscopy, Sir W. **Huggins**, was one of the earliest members of our Society, and rendered good service as Member of Council and Vice-President. His duties as President of the Royal Society, and in other important capacities, prevented his frequent attendance at our Meetings during recent years, but many Members of our Society have profited by his inspiration, and will hold him in grateful remembrance for his kindly advice and assistance.

Mr. H. W. **Eve** joined the Society during its first Session in 1874, and did much to develop the teaching of science at University College School, where some of our Members received their first introduction to physics.

Major P. **Cardew**, R.E., who joined the Society in 1896, as head of the electrical department of the Board of Trade, was closely concerned with the establishment and legal definition of the electrical standards, which are of interest to all physicists, but his principal work as an electrical engineer lay somewhat outside the scope of our Society.

Here follows the special subject of the Address which will be found on pp. 153–189 of the "Proceedings."

February 24th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

The following Papers were read :—

1. On Flames of Low Temperature Supported by Ozone. By Prof. R. J. **Strutt**, F.R.S.
2. On The Movement of a Coloured Index along a Capillary Tube and its Application to the Measurements of the Circulation of Water in a Closed Circuit, was read by Dr. **Albert Griffiths**.
3. An Optical Lever of High-Power suitable for the Determination of Small Thicknesses and Displacements was exhibited by Mr. E. H. **Rayner**.

March 10th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

A Demonstration of the Working of the Anschütz Gyro Compass, illustrated by lantern slides and models, was given by Mr. G. K. B. **Elphinstone**.

A Paper by Dr. W. H. **Eccles**, entitled Note on an Electrical Trevelyan Rocker, and one by Dr. G. W. C. **Kaye**, entitled Some Notes on the Tilted Gold Leaf Electrometer, were taken as read, the apparatus in each case being on the table for inspection afterwards. A Paper entitled The Roots of the Neumann and Bessel Functions, by Mr. J. R. **Airey**, was also presented to the meeting and taken as read.

March 24th, 1911.

Meeting held at the City Guilds Engineering College by kind invitation of Prof. T. **Mather**, F.R.S.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

1. A Sensitive Thermo-Regulator was shown by Dr. H. F. **Haworth**.
2. Some Experiments in the Measurement of Electrolytic Resistance using Alternating Currents were shown by Dr. H. F. **Haworth**.
3. A Paper on Oscillatory Currents in Coupled Circuits was read by Prof. G. W. O. **Howe**.
4. Some Radio-telegraphic Apparatus in Use at the City and Guilds College was briefly exhibited by Prof. G. W. O. **Howe**.

April 28th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

The following Papers were read :—

1. On High Tension Electrostatic Wattmeters. By Prof. E. **Wilson**.
2. On the Behaviour of Incandescent Lime Cathodes. By Dr. R. S. **Willows** and Mr. T. **Picton**.
3. On the formation of Dust Striations by an Electric Spark. By Dr. S. **Marsh** and Mr. W. H. **Nottage**.

A Paper by Prof. E. **Wilson** and Mr. L. C. **Budd** on Previous Magnetic History as affected by Temperature was taken as read.

May 12th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. **Callendar**, F.R.S., President, in the Chair.

1. Some Diagrams of Stream Lines past an Elliptic Cylinder were exhibited by Sir **George Greenhill** and Col. R. L. **Hippisley**.

2. A Paper on The Method of Constant Rate of Change of Flux as a Standard for determining Magnetisation Curves of Iron. By J. T. MORRIS and T. H. LANGFORD, was read by Prof. J. T. MORRIS.

3. An Electric Thermo Regulator was exhibited by the PRESIDENT.

May 26th, 1911.

An informal Meeting was held at the National Physical Laboratory, where a number of demonstrations were given and Fellows had an opportunity for inspecting the laboratories.

June 9th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. CALLENDAR, F.R.S., President, in the Chair.

The following Papers were read :—

1. The Lüders' Lines on Mild Steel. By Mr. W. MASON.
2. A New Method of Harmonic Analysis by Averaging Selected Ordinates. By Prof. S. P. THOMPSON.

The following Demonstrations were given :—

1. A Demonstration of the Subjective Nature of the Difference Tone. By Prof. S. P. THOMPSON.
2. A Demonstration of Spinning Tops and Gyroscopic Apparatus. By Sir GEORGE GREENHILL.
3. A Model Illustrating the Passage of a Light Wave through Quartz was described by Dr. H. S. ALLEN.

The following Papers were taken as read :—

1. The Measurement of Contact Differences of Potential. By Prof. A. ANDERSON and Mr. J. E. BOWEN.
2. A Short Table of Circular and Hyperbolic Functions for Complex Values of the Argument. By Mr. A. JOHNSTONE.

June 30th, 1911.

Meeting held at the Imperial College of Science and Technology.

Prof. H. L. CALLENDAR, F.R.S., President, in the Chair.

1. A Paper on the Capacity Coefficients of Spherical Electrodes was read by Dr. A. RUSSELL.
2. An Exhibition of the Benkö Primary Battery was given by Mr. W. R. COOPER.

A Paper on the Effect of a Narrow Saw-cut in the Edge of a Conducting Strip on the Stream-lines in the Strip and on the Resistance of the Strip, by Prof. C. H. LEES, F.R.S., was taken as read.

I. *The Radio-Balance. A Thermoelectric Balance for the Absolute Measurement of Radiation, with Applications to Radium and its Emanation. By* H. L. CALLENDAR, *M.A., LL.D., F.R.S., Professor of Physics at the Imperial College of Science and Technology, S.W.*

READ JULY 8, 1910. RECEIVED DECEMBER 17, 1910.

1. *Introductory.*—Electric compensation methods, in which the radiation to be measured is directly balanced or compensated by means of heat supplied by an electric current under steady conditions, possess many advantages over the more simple calorimetric methods, which involve the observation of the rate of rise of temperature in a receiver of known thermal capacity. In a report on the measurement of solar radiation presented to the British Association in 1900 (Brit. Assoc. Report, 1900, p. 36) I discussed the general principles of the calorimetric method, and described a form of instrument, termed a radio-calorimeter, specially designed for investigating the errors of the method. Although the method itself was comparatively simple, the observations were tedious and troublesome if a high order of accuracy was attempted, and it was necessary to take account of the effects of lag in the rate of rise of temperature, even when the receiver was a small disc of copper, and the rise of temperature was observed by means of a very sensitive thermo-junction. The chief advantages of electric compensation methods are that they are practically unaffected by lag, since the temperature conditions are steady, and that they are independent of measurements of thermal capacity, which are generally somewhat uncertain.

The best known electric compensation methods are probably those of Angström and Kurlbaum. Angström's pyrheliometer contains a pair of similar strips, each 20 mm. long and 1·5 mm. wide, one of which is exposed to radiation while the other is heated to the same temperature by an electric current. Equality of temperature is indicated by a sensitive galvanometer connected to a pair of copper-constantan thermo-junctions, attached to the backs of the receiving strips, but insulated from them by thin silk paper and shellac varnish. The rate of heat absorption H in watts per square centimetre is given by the simple formula, $H = C^2r/b$, where C is the balancing current in amperes, r the mean resistance per centimetre of the strip in ohms, and b the mean breadth in centimetres. Small differences between the strips are eliminated by exposing either alternately to the radiation to be measured while the other is heated by the current. A small correction is required for the variation of r with temperature. Allowance should also be made for the fact that the temperature of the strip exceeds that indicated by the thermometer, but this correction will not usually amount to more than 2 or 3 parts in 1,000. It is necessary to assume that the resistance of the strip is uniform, since the temperatures are balanced at the centres only. Unfortunately, it is a difficult matter to measure the resistance of so delicate and short a strip satisfactorily, or to verify its uniformity or constancy at any time. The other fundamental measurement, that of the width of the strip b, is also difficult to make on account of the raw edge of smoke black. Owing to the thinness of the strip, about 95 per cent. of the final deflection is produced in the first minute after exposure to radiation, but owing to loss of heat by conduction at the ends of the strip, the temperature at the centre does not become steady to 1 in 1,000 for more than five minutes. The final approach to a steady temperature may be tested most accurately by heating the strip with a constant current, and balancing the E.M.F. of the thermo-couple on a potentiometer. Unfortunately, the instrument cannot be relied on to remain constant in its indications for considerable periods. This want of constancy is most readily detected by comparing the indications of the two strips under the same conditions at different times. In one instrument tested, the difference between the strips increased from 7 to 25 per cent. in the course of two years. Similar differences have been recorded by other observers, and appear usually to be caused by the splitting or separation of the compound strip. A probable source of error is the deterioration of the smoke-black film, the absorption coefficient of which is given as 98 per cent. for a new instrument; but this could hardly account for such large differences between the strips. Perhaps it is too much to expect that so delicate a compound strip, consisting of two different kinds of metallic foil stuck together with shellac and silk paper, should remain constant for many years; but constancy is one of the chief desiderata in an instrument for absolute measurement.

The method of Kurlbaum can be applied to any sensitive bolometer of suitable construction, and avoids many of the difficulties of Angström's method, but introduces others which make it less convenient for measurements of solar radiation, to which it has not generally been applied. The method consists in observing the deflection of the galvanometer when the bolometer of resistance R is exposed to the radiation to be measured, and is traversed by a current C_1. The radiation is then cut off, and the current increased to a value C_2 giving the same deflection. The total heat AH absorbed from the radiation in watts is given by the formula,

$$AH = (C_2^2 - C_1^2)RC_2/C_1.$$

where A is the effective area of the bolometer.

The whole surface of the bolometer may be of the order of 10 sq. cm.; and the difficulty of measuring the width of the strips is avoided by using two grids, the strips of the second being behind the spaces of the first. There is no difficulty in measuring the resistance R at any time, and the grid can be electrolytically coated with platinum black, which is more permanent and more accurately reproducible than smoke-black. On the other hand, it is necessary to balance the bolometer against manganin resistances which are not appreciably affected by the current, and it is therefore impossible, during the absolute measurement, to compensate for changes in the surrounding temperature in the usual way by balancing the bolometer exposed to radiation against a precisely similar grid. The whole change of resistance observed by Kurlbaum was only 3 in 10,000 of R, corresponding to a rise of temperature of 0·1°C. In order to obtain an accuracy of 1 in 1,000 it would be necessary that the temperature of the bolometer enclosure should not change by 0·0001°C. during the observation. In Angström's method the compensation for change in the surrounding temperature is extremely perfect if the strips are equal. In addition to possible errors from change in temperature, the most important sources of error in the bolometric method appear to be (1) the rearward grid of the bolometer is partly shielded from radiation by the front grid, but is equally heated by the current. This will not affect the absolute measurement, provided that the front and back strips are of the same dimensions and resistance, but if they are not equal, the error will be proportional to the shielding and to the difference of resistance. Otherwise uniformity of resistance of the strips in each grid is not so important as uniformity of resistance in the strips of the Angström instrument, because want of uniformity will introduce errors of the second order only in the case of the bolometer, whereas in the Angström instrument the resulting errors may be of the first order, if the resistance of the strip at the centre, where the thermo-junction is attached, differs from the mean value assumed. (2) The area of the bolometer surface which is exposed to radiation may not exactly coincide with that which is heated by the current. This would not matter except near the ends of the strips where they are cooled by conduction, and where radiation and current would differently affect the connections and supports. This end effect is shown by a gradual increase or creep of the deflection due to warming of the end connections and supports, when the bolometer is continuously exposed. The rates of creep are generally different for current and radiation, and some uncertainty is thus introduced into the absolute measurement.

2. *Absolute Recording Bolometer.**

The gradual creep of the zero due to the warming of the end connections and supports is most troublesome when a bolometer mounted on a support of slate or other badly conducting material is continuously exposed to radiation. In order to avoid this source of trouble in the recording bolometer designed for taking records during the total eclipse of Aug. 30th, 1905, a method of compensation was employed which I have adopted in the construction of platinum thermometers for many years. Two similar grids were constructed with strips of different lengths, as indicated in the diagram (Fig. 1) on opposite page. These were similarly mounted side by side, and were connected in opposite arms of a Wheatstone bridge of the type employed in platinum thermometry. The end effects were the same for both grids, and were automatically eliminated. The resistance measured corresponded to the central 2 cm. only of the grid with the longer strips. The effective area of the radiation admitted was determined by a water-cooled diaphragm 2 cm. wide with a sharp edge, corresponding approximately with the difference of area of the grids. This makes the effective area of the bolometer definite, and completely eliminates creep due to the end effect even when the bolometer is exposed all day to full sunshine. For continuous recording, the difference of resistance between the grids is balanced against an equal resistance of the same platinum strip enclosed in the same water-jacket, in order to eliminate the effect of small changes of temperature in the water-jacket. To measure the heating effect of the current, and obtain the absolute equivalent of the radiation, the balancing resistance of platinum strip R is replaced by a constant resistance S, by shifting a plug, and the resistance is balanced without changing the current. The radiation is then cut off, and the current is altered to give the same heating effect as the radiation, under the same conditions. The creep due to the heating effect of the current on the end connections being eliminated. there is

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* The absolute recording bolometer was described and exhibited at a meeting of the Physical Society of London in May, 1905. Publication of the paper was delayed in order to include a reference to the results of the eclipse observations in August. Unfortunately, the manuscript of the paper and all the original observations were lost, together with other luggage, on the way to the observing station in Spain. The paper itself was never printed, although a record of the reading appears in the "Proceedings" of the Physical Society. It did not seem worth while at the time to repeat all the observations and rewrite the paper, because a general account of the instrument and the results obtained during the eclipse were published in the Eclipse Report, in the "Proceedings" of the Royal Society for October, 1905. Moreover, the experiments on the Radio-Balance had already progressed so far as to show that it would prove to be a more convenient instrument in practice. It appeared desirable, however, to include in the present paper a brief account of points not mentioned in the Eclipse Report, in order to explain the difficulties of the method, and to show why it was ultimately abandoned in favour of the radio-balance.

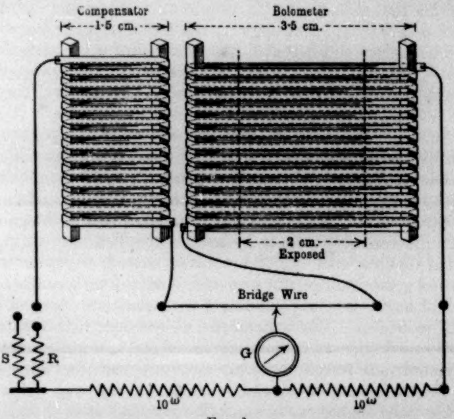

FIG. 1.

no need of hurry, and it is better to balance the resistance in place of employing the deflection method of Kurlbaum. We thus obtain the accurate formula,

$$AH=(C_2^2-C_1^2)R, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where A is the area of the aperture, H the intensity of the radiation absorbed in watts per square centimetre, R the observed resistance (*i.e.*, the difference of resistance of the grids) when the current C_1 is flowing with radiation, or the current C_2 without radiation. Kurlbaum's formula requires the additional factor C_2/C_1, to allow for the change of deflection with current, and neglects the fact that the resistance of the bolometer is different in the two cases for the same deflection. This difference was negligible in his observations, but could not be neglected in applying the method to solar radiation.

An incidental advantage of the end-effect compensation is that it is unnecessary to employ the excessivley thin and fragile Wollaston foil, which is generally used for bolometer grids, but which is difficult to obtain perfectly uniform and free from flaws. Quickness nearly equal to that of the galvanometer is readily obtained by means of rolled wire strip, provided that the end effects are compensated, with the advantage of greater uniformity and ease of adjustment, and with less risk of strain or injury in coating with platinum black or other manipulations. The instrument above described was made with rolled wire strip, carefully tested for uniformity, and was adjusted and tested for compensation over the range 0° to 100°C. by putting steam and ice cold water through the jacket.

The chief objections to the bolometric method in this form are (1) that the determination of the radiation in terms of the electric current requires a separate observation which cannot easily be made simultaneously with the observation of the radiation by a null method. It is therefore necessary to use a water jacket to keep the conditions fairly steady. (2) That the method involves the observation of small changes of resistance, which necessitates the use of somewhat elaborate apparatus, and special attention to details of compensation. In order to meet the objection that the observations of radiation and equivalent heating current in the bolometric method above described are not simultaneous, and to provide some compensation for changes in the surrounding temperature which need not be disconnected during the absolute measurement, I devised and tested various other modifications of the bolometric method, two of which, the four-grid bolometer method and the method of the bright and black bolometer, are of so distinct a type as to be deserving of mention.

3. *Four-Grid Bolometer Method.*

This is a fairly simple method, which gives very good compensation for changes in the surrounding conditions, but in which the end-effects are not exactly compensated. I devised it specially to suit one of Lummer and Kurlbaum's standard pattern bolometers, in which the grids are symmetrically arranged in pairs, the strips of the second grid being behind the interspaces of the first, and those of the third behind the interspaces of the fourth. As usually connected, the first and second grids are in opposite arms of a Wheatstone bridge, of which the third and fourth grids form the other two arms. For the application of the four-grid method, the connections are altered so that the first and second are in parallel, in series with the third and fourth in series (or vice versa, the first and second in series, in series with the third and fourth in parallel). The ratio of the first and second in parallel to the third and fourth in series is approximately 1 to 4, and is balanced by resistances of about 1,000 and 4,000 ohms, forming the other two arms of a bridge. The heating effect of the current C_1 on 3 and 4 in series, is four times as great as on 1 and 2 in parallel. Grids 1 and 2 are exposed to radiation, and the current increased to C_2, till the balance is restored. The connections are then changed to 3 and 4 parallel, and 1 and 2 series; 3 and 4 are exposed to radiation, and the current adjusted to balance as before. This is necessary in order to eliminate small differences between the grids. In each case the actual resistances are measured by taking the ratio to a standard 10 ohm coil in series with the grids, but are not required to more than about 1 in 1,000, the limit of accuracy of the radiation balance. If R is the resistance of each separate grid, C_1 the small current for setting the ratio, C_2 the increased current for balancing the radiation, A the exposed area, a the absorption coefficient, and H the radiation in watts/square centimetre. The approximate equation for the absolute value of the radiation is,

$$aAH=(C_2^2-C_1^2)3R/2, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This equation neglects the small differences between the grids, and the small changes of resistance with heating current, which are easily taken into account in the actual measurement. The currents are measured by the P.D. on the 10-ohm standard. The most important point is to adjust the current C_2 to give the same value of the ratio with radiation, as the current C_1 without.

4. *Bright and Black Bolometer Method.*

A pair of bolometers of equal dimensions and resistance, but one bright and the other black, as indicated in the diagram, Fig. 2, are simultaneously and symmetrically exposed to the radiation to be measured. The difference of temperature between them will be approximately proportional to the intensity of the radiation, and may be utilised for recording the radiation, as in my usual pattern of sunshine recorder. The bright and black bolometers are connected in series with two equal and constant resistances S and S′, of the same order of magnitude as the bolometers, by means of which they can be balanced to equality. They are also connected in parallel with two high resistances P and Q, by means of which the ratio of either bolometer to S or S′ may be determined. If, now, the current through the bolometers is increased while they are exposed to radiation, their resistances may be brought back to equality, and the corresponding rise of temperature deduced from the ratio P/Q. Assuming that the loss of heat by conduction and convection is the same for both bolometers at the same temperature, and that the ratio of the emissive power of either to that of a black body is equal to its coefficient of absorption, the rise of temperature observed when the

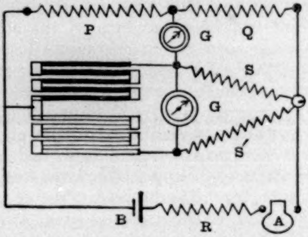

FIG. 2.

resistances are balanced is the same as the rise of temperature of a perfectly black body under the same conditions of exposure to radiation. The intensity of the radiation to be measured can therefore be calculated from the known value of the radiation constant. The function of the electric current in this method is to compensate for the loss of heat by conduction and convection, and the value of the current itself is not required. The rise of temperature observed is the same as it would be in the absence of the current if conduction and convection were also absent. The method would be theoretically perfect if the theoretical conditions assumed were accurately satisfied (as they are no doubt approximately), and if it were practically possible to construct two exactly similar bolometers of equal resistance, one bright and the other black. The theoretical difficulty arises from the fact that the radiation to be measured is generally of a different wave-length and quality from that emitted by the bolometer, and that the coefficient of absorption of a bright metallic surface may be appreciably different for different wave-lengths. The correction for this difference must be separately determined, and gives rise to an uncertainty of the same order of magnitude as the determination of the coefficient of absorption of a single black bolometer. Against the practical difficulty of constructing two similar bolometers may be set the advantage of excellent compensation for end-effects and for changes in the surrounding temperature. When carefully applied the method gives results which agree very well with other methods, but have no great claim to superior accuracy. Like most bolometric methods depending on the observation of small changes of resistance, it is too difficult and delicate for general use. The method works best for low temperature radiation, as from a source at 100°C., since the absorptive and emissive powers of the grids are then very nearly proportional; and the correction is much less important than for a source at a high temperature.

A general objection to all bolometric methods is that on account of the large surface exposed it is necessary to take account of the angular aperture of the receiver as well as the radiant. Even in the case of solar radiation, a bolometer 2 cm. or 3 cm. square, unless provided with a tube of unwieldy length, will include a large and uncertain area of the sky. The difficulty of securing similar exposure is often serious in comparing instruments of different types, especially when one of the receiving surfaces is square or rectangular, like that of a bolometer. On the other hand, the quantity of energy received and available for measurement increases in direct proportion to the surface, so that it is often advantageous to employ a bolometric method for weak sources when a large area is available.

5. *The Disc Radio-Balance.*

The disc form of radio-balance was a natural development of the radio-calorimeter described in the British Association Report, 1900. The latter instrument had a thick copper disc 1·3 cm. diameter, supported by two fine iron-constantan thermo-couples at right angles, either of which could be used for observing the rate of rise of temperature of the disc when exposed to radiation. It occurred to me that by passing an electric current through one of the thermo-couples, the radiation might be directly compensated by the heat absorption due to the peltier effect. The troublesome effects of lag could thus be eliminated, and the radiation deduced in absolute measure from the peltier effect, without a knowledge of the thermal capacity of the disc. The disc was made smaller and thinner, 3 mm. to 4 mm. diameter, and 0·3 to 0·5 mm. thick, to secure greater sensitiveness and uniformity of temperature ; and the radiation aperture was reduced to 2 mm. diameter and placed about 5 mm. in front of the disc. The intensity of radiation which can be directly compensated in this manner by the heat absorption due to the peltier effect with a given instrument is limited in practice by the fact that the current also generates heat C^2R in the wires of the couple, where R is the effective resistance of the couple through which the current is passed. If a is the absorption coefficient of the blackened surface of the disc,

A the area of the aperture admitting radiation in square centimetres.

H the intensity of the radiation in watts per square centimetre,

P the coefficient of the peltier effect in volts,

C the current through the couple in amperes,

$C_0 = P/R$, the neutral current, giving neither heating nor cooling,

when the radiation is directly compensated by the current, we have the equation

$$aAH = PC - C^2R = PC(1 - C/C_0). \quad . \quad . \quad . \quad . \quad (3)$$

The value of the coefficient P is deduced from the observed thermoelectric power of the couple $dE/d\theta$, by means of the well-known relation $P=\theta(dE/d\theta)$, where θ is the absolute temperature. The value of the neutral current C_0 can easily be observed at any time with an accuracy of 1 or 2 parts in 1,000, which is more than ample, since this term is relatively small. The most difficult measurement is the area of the aperture A; and the most uncertain factor, the absorption coefficient, a.

In order to compensate accurately for changes in the surrounding temperature, and to avoid the necessity for a water-jacket, two similar discs with similar connections were mounted side by side in a hollow copper cylinder, 5 mm. thick. The connections for such a pair of discs are shown in the diagram, Fig. 3. The discs of this particular instrument were 3 mm. diameter, and the current for heating or cooling either disc was carried by four pairs of constantan-iron wires, 0·2 mm. diameter, in order to reduce the effective resistance. Either of these could be connected by a double-pole switch to the circuit consisting of battery B, rheostat R, and milliammeter A, for supplying, regulating, and measuring the current. The difference of temperature between the discs was indicated by the sensitive galvanometer G, connected to a single iron-constantan couple with one junction in either disc. The value of the peltier coefficient was 0·01493 volt at 15°C. The values of the neutral currents for the two discs were 0·791 and 0·793 ampere respectively, at 15°C. Taking $a=1$, and $A=0\cdot0320$ sq. cm., the current required to compensate radiation of intensity 1·0 calorie per square centimetre per minute, or 0·070 watt

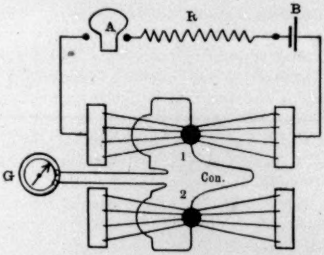

FIG. 3.

per square centimetre, is approximately $C=0{\cdot}200$ ampere, which is about a quarter of the neutral current. The maximum radiation which can be directly compensated by the cooling effect occurs when C is half the neutral current, and is $H=0{\cdot}0925$ watt per square centimetre, or 1·32 calorie per square centimetre per minute. The range could be extended if desired by reducing the resistance R, or the size of the aperture A. But the above range was suitable, and sufficient for most purposes with direct sunshine. With a sensitive galvanometer of the suspended coil type a deflection of the order of 500 mm. to 1,000 mm. at 1 metre for 1 calorie per square centimetre per minute could be obtained. Several different combinations of discs and wires were tried. With one pair of wires in place of four in parallel for the current the effective resistance was increased four times, and the range of direct compensation reduced to one quarter. But the sensitiveness was nearly doubled, so that this combination was more suitable for weak sources of radiation. The accuracy of the theoretical formula (3) was carefully verified by measuring both the current and the E.M.F. of the couple on a potentiometer, over the whole range up to the neutral current. The method was also tested at various temperatures up to 100°C. by means of a water-jacket. The temperature coefficient of the neutral current with the particular couple employed was found to be very nearly the same as that of the peltier effect. The coefficient of C^2 in equation (3) was thus practically independent of the temperature.

6. *The Radio-Balance as a Pyrheliometer.*

Fig. 4 shows the radio-balance arranged as a pyrheliometer. The thick copper cylinder containing the discs and their connections is extended by a tube containing suitable diaphragms for limiting the exposure. The tube is mounted equatorially, so that the sun can be followed by a single worm wheel on the polar axis. The whole is mounted on a levelling stand, with level and compass for the approximate adjustment of the polar axis. The tube is gilt in place of being lacquered. The gilt stands exposure to sunshine better than lacquer, and also greatly diminishes the absorption of heat. There are no external terminals, which may be a source of trouble, especially in an instrument which has to be exposed to full sunshine. Connections are made to the measuring apparatus by means of three pairs of flexible leads, one for the galvano-

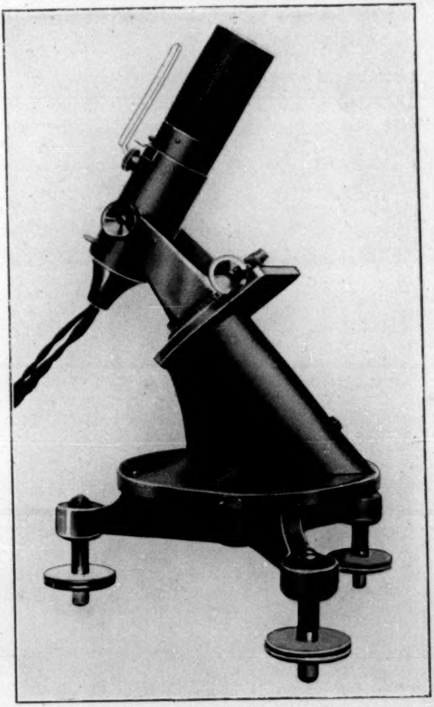

FIG. 4.

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meter, and one for each of the two discs. The latter are connected to the double pole switch for conveying the current to either disc. The leads are permanently connected to the couples inside the copper cylinder, and are seen in the figure issuing through an ebonite thimble at the base of the tube.

The chief advantage of the direct compensation method, especially when the radiation to be measured is variable, like sunshine, is that the result is obtained by a single observation on one disc, and that it is unnecessary to upset the conditions and adjustments by interchanging the discs. A subsidiary advantage, which is quite appreciable when a portable milliammeter is used for measuring the current, is that a very open current scale may be obtained in the neighbourhood of the radiation to be measured. Thus, with the constants already given we obtain the following scale for the radio-balance, taking $a = 0{\cdot}98$:—

Current in amperes	0·050	0·100	0·150	0·200	0·250	0·300	0·350
Radiation, cals/cm.^{2}	0·312	0·581	0·811	0·995	1·140	1·242	1·300
„ C^2 scale	0·063	0·250	0·563	1·000	1·563	2·250	3·063

An error of 1 per cent. in the current between 200 and 300 milliamperes is equivalent to an error of only one-half of 1 per cent. in the radiation. Whereas on the C^2 scale of the bolometer or Angström pyrheliometer, an error of 1 per cent. in the current would be equivalent to an error of 2 per cent. in the radiation. Since few portable milliammeters are correct to more than one half of 1 per cent., it is a distinct gain to be able to make the accuracy of this measurement four times less important .

When the intensity of solar radiation is rapidly variable, as is often the case near London, even when the sky appears to be quite clear, the most convenient method of observation is to set the current at a suitable constant value, such as 0·200 ampere, corresponding to nearly 1 calorie per square centimetre per minute, and to observe the deflections of the galvanometer, which are proportional to the changes in the intensity of the radiation. The scale of the galvanometer must in this case be tested under the conditions of the experiment, preferably by reversing a known small current C through the disc, and observing the corresponding deflection D on reversal, which is equivalent to a heat supply of 2 PC.

A disadvantage of the radio-balance is that the increase of the peltier effect per 1°C. for the couples employed is very nearly 0·0001 volt in 0·0150 volt. It is necessary to read the temperature to 0·1°C. (by means of the attached thermometer shown in Fig. 4, the bulb of which is inside the copper cylinder), in order to obtain an accuracy of the order of 1 in 1,500. For this reason the instrument should be allowed to attain an approximately steady temperature after exposure to sunshine, before observations are taken.

The radio-balance may also be employed in the same manner as the Angström pyrheliometer, by exposing one of the discs to radiation, and heating (instead of cooling) the other disc by the current. The range of compensation in this case is unlimited, since the joule effect acts in the same direction as the peltier effect. It is merely necessary to change the negative sign in equation (3) to positive. The disadvantage of this method is that errors in the current measurement become more important, and that it is necessary to interchange the discs, as in the Angström method, in order to eliminate small differences between them, which, however, should not amount to more than 1 per cent.

It is an advantage that the black coating on the disc, which is liable to deteriorate by exposure, can readily be renewed at any time without risk of injury to the couples. The discs may be cleaned with spirit, and coated with a thin film of black shellac varnish. They are then smoked, the wires being protected by a card with holes through which the discs protrude. The double film of varnish and smoke-black adheres better than smoke-black alone, and gives a higher absorption coefficient, approximately 99 per cent. for sunshine. To recoat the delicate strips of a bolometer, or of an Angström pyrheliometer is a far more difficult and hazardous undertaking.

The most obvious error possible in the construction of the disc radio-balance is that, if the wires of the couple connected to the galvanometer are not symmetrically situated with respect to the wires conveying the current through the disc, there will be a difference of potential introduced into the galvanometer circuit, which will be equivalent to an error in the peltier effect. The possible limits of this error can be tested by connecting the two constantan wires to the battery and the two iron wires to the galvanometer. This arrangement eliminates the peltier effect, but makes the flow of current through the disc such as to produce the greatest possible disturbance in the galvanometer circuit. All the discs were tested in this way, and it was found that the galvanometer deflection was only 5 to 6 per cent. in this extreme case of the deflection due to the peltier effect when the flow was symmetrical. The possible error when the wires were connected in the usual way for the peltier effect could be estimated from this observation, and did not amount in any case to more than 1 in 1,000.

The fundamental assumption involved in the method is that the temperature of the copper disc is practically uniform, owing to its small size and high thermal conductivity. Several different methods were tried of connecting the wires to the discs, but without appreciable variations in the results. The method finally adopted, as being the simplest, was to punch with a fine needle a number of holes equidistant from the centre of the disc and evenly spaced. The ends of the iron and constantan wires were inserted in these holes and simultaneously sweated in place by floating a small quantity of solder over the back of the disc. Discs prepared in this way gave very uniform results, and were easily reproducible with a high degree of accuracy.

Owing to the relatively large mass of the copper disc, as compared with a bolometer film which may be a hundred times thinner, the first movement of the galvanometer, when the disc is exposed to radiation without simultaneously applying the balancing current, is not so rapid as in the case of a bolometer for an equal final deflection. But owing to the absence of end-effects, and to the perfect compensation for changes in the surrounding temperature, the radio-balance reaches its final steady deflection to 1 in 1,000, quite as quickly as the bolometer, and in most cases with greater precision and regularity.

7. *The Cup Radio-Balance.*

The disc radio-balance is comparatively easy to construct and to reproduce, and is very suitable for observations of solar radiation, but cannot be applied to measurements other than radiation. The sensitiveness is limited to that of a single thermo-couple, and the coefficient of absorption of the disc is an uncertain factor, though probably less uncertain than in most other methods. In order to diminish the uncertainty of the coefficient of absorption, the receiving surface was made in the form of a cup, 3 mm. to 4 mm. diameter and 8 mm. to 10 mm. deep. It was found that, if the interior of the cup was not blackened, but left as it came from the spinning, its effective coefficient of absorption was 94 to 96 per cent. as against 30 per cent. for a flat disc in a similar condition. It was a fair inference that if the interior of the cup were blackened, its coefficient of absorption would be practically equal to 100 per cent. In order to increase the sensitiveness, the cup was mounted in a tubular pile, as indicated in the sectional diagram, Fig. 5. The upper junctions of the pile were bound firmly round the middle of the cup, from which they were insulated by thin paper and shellac. The

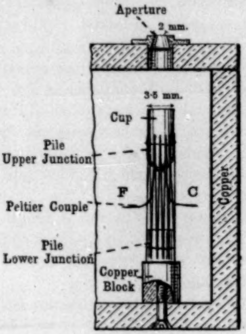

Aperture
2 mm.
3·5 mm.
Cup
Pile
Upper Junction
F
C
Copper
Peltier Couple
Pile
Lower Junction
Copper
Block

FIG. 5.

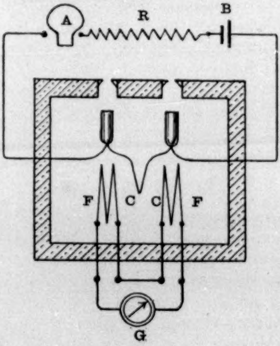


FIG. 6.

lower junctions were similarly fastened to a small copper block, screwed to the base of the thick copper cylinder, in which a second similar pile and cup was enclosed to secure the necessary compensation for changes in the surrounding conditions. This arrangement is indicated in the diagram of connections, Fig. 6, in which, however, the cups are shown separated from the piles, in order to facilitate the tracing of the connections, which are the same as for the disc radio-balance, except that the piles are insulated from the cups. The peltier couples for compensating the radiation are single couples soldered ot the bottoms of the cups. The four pairs of leads for the two couples and the two piles are brought out separately, so that the insulation can be readily tested at any time. The piles in most of the instruments constructed on this plan consisted of 12 couples each, and gave about seven times the sensitiveness of a single couple. This could easily be tested by exposing one of the cups to radiation and observing the deflections (1) when the piles were connected in the usual way, (2) when the single peltier couple was connected to the galvanometer in place of the piles.

8. *Conditions of Sensitiveness.*

It may be of interest to give the theory on which the construction of the piles was based, as it has often proved useful in similar cases in the design of thermoelectric apparatus. The heat supply W in watts required to raise the temperature of the cup 1°C. above its surroundings, is equal to the rate of heat loss a from the cup by radiation and convection (including conduction loss through the peltier couple) per degree rise of temperature, together with the rate of loss by conduction through the wires of the pile itself. The latter loss will be directly proportional to the number of couples n, and inversely proportional to the resistance R of each couple, so that it may be represented by a term of the form bn/R. The deflection D of the galvanometer, of resistance G, per degree rise of temperature of the cup, will be directly proportional to the number of couples n, and inversely proportional to the whole resistance $G+nR$. The watts per scale division of deflection W/D, which is to be a minimum, may be written,

$$W/D=(a/n+b/R)(G+nR)=aG/n+bn+bG/R+aR,$$

This will evidently be a minimum when $n=\sqrt{aG/b}$, and $R=\sqrt{bG/a}$, which gives the appropriate number of couples and the resistance of each. The dimensions of the wires within reasonable limits are immaterial, provided that the resistance is the same. The above conditions are equivalent to $G=nR$, and $a=bn/R$; or the whole resistance of the pile should be equal to that of the galvanometer, and the loss of heat by conduction through the pile should be equal to the sum of the other losses from the cup. A preliminary estimate of a and b gave 12 as a suitable number of couples in each pile, employing the same iron-constantan wires 0·2 mm. diameter of suitable length, with a total resistance of 4·8 ohms and a 5 ohm galvanometer. Other piles with different wires and different numbers of couples were subsequently made in order to test the theory, but it was found that the preliminary estimate was so nearly correct that no advantage was gained in sensitiveness, although the experiments were useful as throwing a good deal of light on other details of construction. Thus it was found to be important to mount the lower junctions of each pile on a copper block fastened to the cylinder, and not on an insulating block, as might appear better and simpler at first sight. In the latter case the lower junctions are heated by conduction along the pile; the deflection reaches a maximum in about a minute and then diminishes slightly; and the final deflection is smaller and less regular than with the copper block mounting, owing to the retention of heat by the insulating block.

9. *Method of Using the Cup Radio-Balance.*

The cup radio-balance may be used for measuring radiation in the same way as the disc radio-balance, by heating or cooling either cup separately by means of the peltier couples, the piles in which the cups are mounted being oppositely connected in the galvanometer circuit so as to indicate the difference of temperature between the cups. In this method the equation previously given applies, the neutral current must be determined, and the range of direct compensation by the cooling effect is limited. Since, however, the piles are necessarily insulated from the cups, it is more difficult to secure exact equality of sensitiveness with the piles than with the single couples soldered to the discs of the disc pattern, and it is desirable to interchange the cups at each observation. For this reason, and also to secure greater range and exact elimination of the joule effect, it is more convenient with the cup balance to proceed in a different way. The peltier couples are connected in opposition, and the same current is passed through both, so that the cup exposed to radiation is cooled while the other is heated by the peltier effect. (This cannot be done with the disc balance because the galvanometer couple is soldered to the discs.) The current is adjusted to give an approximate balance. The other cup is then exposed to radiation, and the current simultaneously reversed without altering its value. The sum of the opposite deflections (or the algebraic difference of the deflections) gives the defect of balance, which can always be made very small, and is easily reduced to watts, since the sensitiveness of the galvanometer s in scale divisions per watt is readily determined at any time, and remains very nearly constant. If D′, D″ are the deflections of the galvanometer when cups 1 and 2 respectively are exposed, and the radiation is balanced by a current C through both peltier couples, the equation for the radiation H is

$$AH=2PC+(D'-D'')/s, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

A is the mean of the areas of the two apertures in front of the two cups. D′—D″ is the movement of the spot of light in scale divisions measured in the same direction as the movement produced by switching over radiation from cup 1 to cup 2. If the current is too large the movement will be in the opposite direction, and D′—D″ will be negative. The sensitiveness s is found by observing the deflection d due to reversal of a small current c through the peltier couples, which gives $s=d/2Pc$. Since it is not immediately obvious that the above method simultaneously eliminates the joule effect and compensates for inequality between the piles, the following more general investigation is given :—

10. *Elimination of the Joule Effect and Inequality of Piles.*

If W′, W″ are heat sources in watts in cups 1 and 2 capable of being interchanged,
P′, P″, coefficients of the peltier effect for couples soldered to cups 1 and 2,
R′, R″, effective resistances of peltier couples for cups 1 and 2.
$P=(P'+P'')/2$=mean of peltier coefficients.
s', s'', deflections in scale divisions per watt supplied to cups 1 and 2.
$s=s'+s''$=total deflection per watt transferred from cup 1 to cup 2.

D' observed deflection with W' in 1 and W'' in 2, balanced by a current C cooling 1 and heating 2, measured in the direction due to heat in 1.

D'' the corresponding deflection measured in the same direction when W'' and W' are interchanged, and the same current C is reversed.

Collecting the quantities of heat supplied to either cup, we have

$$s'(W'-CP'+C^2R')-s''(W''+CP''+C^2R'')=D'.$$

$$s'(W''+CP'+C^2R')-s''(W'-CP''+C^2R'')=D''.$$

Taking the difference between these two equations, the joule effects are exactly eliminated even if s', s'', and R', R'', are widely different, provided that the current C is unaltered on reversal. We thus obtain

$$(s'+s'')(W'-W'')-2C(P's'+P''s'')=D'-D''.$$

Since P', P'' are always very nearly equal, and s', s'' rarely differ by so much as 2 or 3 per cent., we may neglect the product of the two small quantities $(s'-s'')$ and $(P'-P'')/2$, and write $(P's'+P''s'')=Ps$, which gives the equation,

$$W'-W''=2PC+(D'-D'')/s.$$

The two piles must always be connected in opposition in the galvanometer circuit in order to eliminate the effect of external disturbances. It is important for this reason that the piles should be nearly equal. The values of s', s'' are not required separately, but their equality may be tested by reversing the same current C through each of the peltier couples separately, and comparing the corresponding deflections, which are proportional to $P's'$, and $P''s''$. It may be assumed that P' and P'' are very nearly equal, but these cannot be tested without using a separate source of heat as explained in the next section. It is, of course, most important to test the insulation of each pile from the copper cylinder, and from the peltier couples soldered to the cups. This test can be made very easily at any time.

11. *Verification of the Peltier Effect.*

A special advantage of the cup form of radio-balance over the disc form is that the actual value of the peltier effect can be verified for either couple at any time after the apparatus has been made by placing in the cups the two junctions of a standard couple, the electromotive power of which has been directly determined. The junctions of the standard couple are attached to small blocks or cups of copper closely fitting the cups of the radio-balance. The deflection due to the standard couple should be very nearly the same as that due to the couples of the balance, if the wires for both couples are from the same reels. In making the test one junction must be inserted in each cup in order to compensate for external disturbances due to conduction of heat along the wires. The standard couples must be in place, but disconnected from each other, when the deflection due to the peltier couples of the balance is observed. The cups can be tested separately, for equality of P' and P'' by reversing the current through one of the junctions only, but both junctions must be in place. The equality of s' and s'' can be similarly tested. If any difference is observed bewteen P' and P'' it is generally due to defective soldering, or bad insulation. Such differences might arise from want of uniformity of the constantan wire. If the wire is not uniform it is no good for thermoelectric work.

The radio-balance is well suited for measuring the peltier effect for any other thermo-junction by comparison with the standard, or for verifying the fundamental relation $P=\theta(dE/d\theta)$. This may be most simply effected by using a small resistance coil as a source of heat in one of the cups, and balancing the heat generated by means of the peltier couples; but it is most essential to insert in the other cup a pair of compensating leads similar to those of the resistance coil and traversed by the same current, in order to balance the heat generated in the leads, and the conduction along the leads. The compensating leads should also have a dummy coil wound on them in order to balance the thermal capacities. A still more accurate method is to use a thermo-couple for the leads of the resistance coil and the compensating leads, and to find the current required to balance the joule effect in the coil against the peltier effects in the leads. The result is then compared with the thermoelectric power of the couple as directly measured for the same combination.

I have tried various other modifications of the same method, and find there is no great difficulty in verifying the relation to nearly 1 in 1,000 with the radio-balance. It would be very difficult to obtain anything like this order of accuracy with a calorimetric method, unless a similar balance or compensation method were employed. In Jahn's observations with a Bunsen ice calorimeter (" Wied. Ann." 34, p. 755, 1888) the peltier effect was only a small fraction of the total effect observed, and the discrepancies between theory and observation were of the order of 20 per cent. With the radio-balance, the joule effect and conduction of heat along the wires are only a small fraction, about 5 per cent. of the peltier effect, and are accurately compensated. The observation of the peltier effect can be made in 10 minutes, and is more easy and accurate than the observation of the thermoelectric power.

12. *Method of Taking Observations.*

One of the chief difficulties in comparing different instruments for measuring radiation is that of securing a constant and reproducible source of radiation. For this purpose I employed the same 90-volt 100 c.p. focus lamps as were used in the investigation of the radio-calorimeter (B.A. Report, 1900). These give a fairly intense radiant of about 2 cm. square area, and have remained extremely constant, being generally run at between 70 and 80 volts. The voltage was adjusted either by means of a Weston laboratory standard voltmeter, accurate to about 1 in 1,000, or by an electric recorder working on the potentiometer method with a scale of 2 cm. per volt. The current was supplied by a large storage battery, and the radiation generally became constant in about 10 minutes. Owing to the irregular shape of the filament of the lamp, the distance from the mean plane of the filament could not be measured nearer than about 1 mm., but by keeping the lamp fixed in position the distances of the different instruments to be compared from the lamp could be adjusted to about 0·1 mm. by the aid of a fixed telescope. With 80 volts on the lamp an intensity of nearly 1 calorie per square centimetre per minute could be obtained at a distance of 20 cm. At this distance a variation of 1 mm. in the distance would be equivalent to an error of 1 per cent. in the radiation, so that it was necessary to adjust the distance to 0·1 mm. A more serious difficulty was that a variation of 1 per cent. in the voltage produced a variation of nearly 3 per cent. in the radiation, so that the radiation could not be reproduced with certainty to less than 2 or 3 in 1,000.

In the earlier experiments in 1905-6, a very sensitive galvanometer with a suspended magnet and mirror system was employed for observing or balancing the E.M.F. of the thermocouple due to difference of temperature between the discs.

Owing to the excessive sensitiveness of the galvanometer, and to magnetic disturbances due to electric railways, &c., in the vicinity, the E.M.F. of the couple could not be observed by the deflection method, and it was necessary to balance it on a special form of potentiometer of the type described in the Eclipse Report (*loc. cit.*). The current through the peltier couples was also measured by observing the potential difference on a standard resistance with the same potentiometer, which could readily be adjusted to suit a range of either 50 microvolts for the couple or 50 millivolts for the current. Owing to increasing magnetic disturbances due to the electrification of the Metropolitan and District Railways, it became necessary to employ galvanometers of the suspended coil type. Two of these, specially designed for thermoelectric work, with resistances of 13 and 7 ohms respectively, giving deflections of the order of 10 mm. to 20 mm. at 1 metre per microvolt, were obtained from the Instrument Co., Cambridge. A separate potentiometer, designed by the same makers, with a range of 30 millivolts reading to 1 microvolt, was used for the peltier currents, as it was found to be inconvenient to use one galvanometer and potentiometer for both purposes. A standard 1 ohm coil was used for currents up to 30 milliamperes, and a standard 0·1 ohm coil for currents from 30 to 300 milliamperes. Currents down to 3 milliamperes could be measured with an order of accuracy never less than 1 in 3,000. Currents above 300 milliamperes were seldom required, but were usually measured with a Weston laboratory standard ammeter. The galvanometer deflections on the balancing thermo-couples or piles could be read without difficulty to 0·1 mm., corresponding to 0·01 microvolt or less. Measured in terms of rate of heat supply, the sensitiveness obtainable with the moving coil galvanometers was about 1 to 2 scale divisions per microwatt for the cup radio-balances, and 2 or 3 microwatts per scale division for the disc balances. In the majority of the observations the heat supply was balanced by means of the current through the peltier couples, so that the residual deflection to be observed was usually very small. But if the deflection did not exceed 250 mm. on either side of zero, it was found that an accuracy of 1 in 1,000 could be obtained by observing the deflections, provided that readings were taken in a regular order at regular intervals. This was often useful as a check on the working of the apparatus.

Of the many hundreds of observations taken in testing the apparatus, it will suffice to give two or three specimens in order to illustrate different methods of observation and comparison. The first illustration is a comparison of two different disc balances B and C, with an Angström pyrheliometer at the same distance from the focus lamp at approximately the same voltage. Radio-balance B was a laboratory pattern instrument with water jackets, and had discs 3 mm. in diameter. Radio-balance C was mounted as a pyrheliometer, the discs were 4 mm. in diameter, and the wires were differently arranged, so that the values of the neutral currents were different for B and C. Observations were taken by deflection on radiation and current separately, as well as by direct balance. In the balance method, equation (3) applies, and it is necessary to know the neutral current, since the joule effect involves a correction of 10 to 15 per cent. In the deflection method, readings are taken with the current direct and reversed through either disc. The deflections on either side are unequal, but the joule effect is eliminated from the algebraic difference.

The values of the thermoelectric power $dE/d\theta$ in microvolts per 1°C., and of the peltier coefficient P in microvolts, were as follows for the samples of wire employed :—

Temp. C.	0°	5°	10°	15°	20°	25°	30°	35°	40°
$dE/d\theta$...	49·60	50·34	51·09	51·83	52·58	53·32	54·07	54·81	55·56
P.........	13,550	14,000	14,465	14,930	15,410	15,897	16,390	16,890	17,397

The currents C in the following comparisons are stated in milliamperes, and the heat supply W in microwatts, as being the most convenient units. The corresponding values of the radiation intensity H in watts per square centimetre are deduced by dividing W by the areas of the apertures in square centimetres and are reduced to calories per square centimetre per minute by the factor 60/4·18.

Radio-Balance B. Mean aperture 0·03031 *sq. cm. Neutral current* 0·804 *amp. at* 22°*C.*

—	Balance method.		Deflection with current alone.				Radiation alone.	
Disc. exp. ...	(1)	(2)	(1)	(1)	(2)	(2)	(1)	(2)
Temp. C.......	22·1°	22·4°	20·8°	20·7°	20·6°	20·6°	20·9°	21·3°
Volts	74·65	74·75	—	—	—	—	74·74	74·70
Current	−92·03	+92·00	−79·07	+78·95	−78·88	+78·83	—	—
Deflection ...	− 1·1	− 1·3	−187·0	+227·4	−226·8	+180·6	+219·0	−209·0
Microwatts .	1,270	1,277	1,223=414·4 sc.		1,220=407.4sc.		428·0 sc.=1,272 mw.	

Mean result:—Radiation at 74·72 volts=1,273 mw.=0·04200 w/cm.²=0·6029 cal./cm.² min.

A positive sign for current or deflection corresponds to evolution of heat in disc (1) or absorption of heat in disc (2), a negative sign the reverse.

Radio-Balance C. Mean aperture 0·03211 *sq. cm. Neutral Current* 0·702 *and* 0·698 *amp.*

—	Balance method.		Deflections with current alone.				Radiation alone.	
Disc exp.	(1)	(2)	(1)	(1)	(2)	(2)	(2)	(1)
Temp. C.......	22·0°	22·3°	22·4°	22·3°	22·0°	21·7°	22·3°	22·5°
Volts	74·64	74·58	—	—	—	—	74·65	74·55
Current	−99·92	+99·85	−79·48	+79·40	−79·30	+79·26	—	—
Deflection ...	− 9·2	− 9·6	−192·2	+224·8	−243·0	+177·6	−235·0	+217·3
Microwatts .	1,338	1,339	1,242=417·0 sc.		1,236=420·6 sc.		452·3 sc.=1,338 mw.	

Mean result :—Radiation at 74·60 volts=1,338·5 mw.=0·04168 w/cm.^{2}=0·5982 cal./cm.^{2} min.
Reduced to 75·0 volts B gives 0·6093 cal./cm.^{2} min. C gives 0·6078 cal./cm.^{2} min.

The difference is within the limits of error of the voltage, but might equally be due to a slight difference in the coefficients of absorption.

Angström Pyrheliometer. $H=7{\cdot}17\ C^2$ (*C in amps.*) *Smoke-black assumed* 98 *per cent.*

—	Radiation alone.		Balance method.		Current alone.	
Strip Exp.	(1)	(2)	(1)	(2)	(1)	(2)
Temp. C.	22·3°	22·4°	22·2°	22·4°	22·5°	22·3°
Volts	74·8	74·6	74·6	74·5	—	—
Current m.a. ...	—	—	−289·0	+255·7	+255·2	−288·7
Deflection	+243·0	−228·5	−2·5	−2·0	+229·0	−252·5
Cal./cm.^{2} min. ...	0·494	0·541	0·599	0·469	D/C^2=3,530	3,030

In using the Angström pyrheliometer by the balance method in the usual way, when strip (1) is exposed, the current is passed through strip (2), and vice versa. This tends to exaggerate any difference between the strips, and gives in the above example results differing by 24 per cent. The difference is partly due to a difference of sensitiveness of the thermo-junctions, caused by a slight difference in the attachment to the strips. This difference may be eliminated by comparing the deflection produced by the radiation with the deflection produced by the current *through the same strip*, which gives the more concordant results shown in the columns headed " radiation alone " ; but this method cannot be used to obtain a balance. Both results are much too low, and it seems probable that the resistance of the strips has increased and the film of smoke-black deteriorated. The strips were not visibly split or separated, but an almost imperceptible defect of this kind generally reduces the readings considerably. The current deflections showed a marked tendency to creep, but were within 1 per cent. of their final values at the end of three minutes when the readings were taken.

With the cup radio-balance, since the cups were insulated from the piles, it was possible either to compensate the radiation by the heat absorption due to a negative current in the peltier couple soldered to the exposed cup (current through one cup only), or to pass the same current through both cups, cooling the cup exposed to radiation and heating the other to balance. Both methods are illustrated in the following comparison, which also illustrates the effect of blacking the insides of the cups.

Radio-Balance D. Neutral Current 0·380 *amp. Mean aperture* 0·3199 *sq. cm.*

—	Cups clean.				Cups blackened.			
	Current through exposed cup.		Both cups.		Both cups.		Current alone.	
Cup. exp. ...	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Temp. C. ...	19·62	19·68	20·18	20·22	19·58	19·62	19·68	19·70
Volts	75·72	75·68	75·45	75·35	75·20	75·10	—	—
Current	−103·52	+103·50	−37·55	+37·51	−38·35	+38·61	+ 8·060	−8·058
Deflection ...	− 4·2	− 4·8	− 6·3	− 8·3	−11·5	− 2·3	+88·5	−106·5
Microwatts .	1,172 at 75·70		1,155 at 75·40		1,199 at 75·15		195sc.=248mw.	
At 75 volts...	1,139 mw.		1,137 mw.		1,192 mw.=0·03725 watts/sq.cm.			

The ratio 1,138/1,192 of the results with the cups clean and blackened respectively gives 96 per cent. as the effective absorption of the clean cups. A similar experiment with radio-balance C (disc pattern), gave 26 per cent. as the effective absorption of the discs when clean but not polished. When coated with thick indian ink black, the coefficient of absorption of the disc was 94 per cent. With thin black shellac varnish, opaque but rather shiny, the absorption was only 91 per cent. When the shellac varnish was coated with smoke, which gave a very intense dead black, the absorption rose to 99 per cent. as tested by comparison with radio-balance D, assuming total absorption for the blackened cups. The disc radio-balance B, similarly varnished and smoked, also gave nearly the same result when compared with D.

Readings were taken at the same distance under the same conditions with the compensated bolometer with water in the jackets at 17·70°C.

Difference of bolometer and compensator $R_1=9{\cdot}0390$ ohms, with $C_1=38{\cdot}03$ m.a.

Radiation from lamp at 75·05 volts, $R_2=9{\cdot}2841$ ohms, with $C_1=38{\cdot}01$ m.a.

Screened from radiation, but current increased to balance at R_2, $C_2=114{\cdot}56$ m.a.

Exposed area 3·002 square cm. Radiation=0·03612 watt per square centimetre.

Compared with the above value given by radio-balance D, this result would make the absorption of the platinum black on the bolometer slightly less than 97 per cent., which is not improbable, as the black was rather old and had probably deteriorated.

13. *Application to Radium and its Emanation.*

The cup form of radio-balance is very suitable for measuring small quantities of heat, such as that evolved by a milligram of radium. The specimen first tested was an old sample of impure radium bromide, supposed to be about 5 milligrams, which had been obtained about five years previously. It was sealed in a glass tube 3 mm. in diameter, and 4 cm. long. This could easily be transferred from one cup to the other, and the deflection observed, or compensated by the peltier couples. After several readings had been taken with fairly consistent results, giving 300 microwatts as the rate of development of heat, the sample was sent by post to Prof. Rutherford, who kindly undertook to compare it with his standards. He found the sample equivalent to 3·40 milligrams of pure anhydrous radium bromide, or to 1·99 milligrams of radium. On its return, the heating effect of the sample was found to have fallen to 267 microwatts, but it regained its old value in less than a month. These changes were subsequently traced to the escape of some of the emanation from the radium bromide, which was found to be not quite dry. Any emanation which escaped would diffuse through the tube, so that its heating effect would be lost to the small cup at the bottom. The cylindrical tube was unsuitable for an accurate determination on account of the relatively large loss of heat by conduction along the tube. The greater part of this radium sample was subsequently enclosed in a small glass bulb more nearly fitting the cup of the radio-balance, and the values of the heating effect, when the sample had recovered from the de-emanation, were found to be extremely constant.

Prof. Rutherford also very kindly prepared and sent two samples of emanation sealed in small glass bulbs fitting the cups of the balance. The first of these, by comparison with his standards, corresponded with the equilibrium amount from 25·8 milligrams of $RaBr_2$ at noon on May 31st. This value was verified on its arrival by comparison with the radium sample already tested by Prof. Rutherford. The heating effect of the emanation was tested daily for a fortnight, and at longer intervals subsequently, and was found to decay at the same rate as the γ-ray activity, falling to half value in 3·86 days. The mean deviation of the observations from the theoretical curve was only 0·2 per cent. The relative values of the heating effect could be measured in the early stages without difficulty to 1 in 1,000, but the accuracy attainable began to diminish when the heating effect had fallen to 200 microwatts.

The second sample of emanation arrived on July 7th, and was employed to demonstrate the heating effect of the emanation with the radio-balance at the meeting of the Physical Society on July 8th. Its heating effect at that time was 1,300 microwatts, equivalent to a deflection of 1,000 mm. at 1 metre with radio-balance E and the 7 ohm galvanometer. The value of this sample was given by Prof. Rutherford as being equivalent in γ-ray activity to 35·2 milligrams of $RaBr_2$ at noon on July 6th. It was compared on July 7th, 8th and 9th with the old sample of radium, which had remained sealed in its original glass tube. Assuming the radium to be equivalent to 3·40 milligrams of $RaBr_2$, the comparisons with the electroscope by means of the γ-rays agreed in making the emanation equivalent to 30·6 milligrams of $RaBr_2$ at noon on July 6th. A comparison of the heating effect observed on the same days with that of the first sample of emanation, assumed equivalent to 25·8 milligrams of $RaBr_2$ on May 31st at noon, made the value of the second sample equivalent to 30·4 milligrams of $RaBr_2$ at noon on July 6th. The origin of the discrepancy between Rutherford's value 35·2 for the second sample of emanation, and the values 30·4 and 30·6 obtained here by comparison with the first sample and with the radium, cannot now be traced, and tends to throw some doubt on the absolute values deduced for the heating effect of the radium and the emanation.

In order to obtain a more accurate comparison of the relative values of the heating effect of the radium and the emanation, the greater part of the radium was sealed, on July 9th, in a small glass bulb of the same size as the bulb containing the emanation. The radium was dried and almost completely de-emanated in the process, owing to the small size of the bulb. Its heating effect fell to a minimum of 71·3 microwatts in about four hours. Comparisons of the heating effect with the radio-balance, and of the γ-ray activity with the electroscope were made daily between the radium and the emanation. It was found that the heating effect of the radium could be represented by a nearly constant quantity 70 microwatts plus the heating effect of a quantity of emanation equivalent to its γ-ray activity. The value finally attained by the radium was 255·4 microwatts, of which 70 microwatts, or 27·4 per cent., was apparently due to the radium alone, and the remainder 72·6 per cent. to the emanation in radio-active equilibrium with it. This is probably not quite accurate, because the radium, being an old sample, should have contained an appreciable proportion of polonium, which would affect its evolution of heat to a somewhat uncertain extent. Moreover, the rate of recovery observed did not accurately coincide with a period of 3·86 days, but was appreciably slower, and the radium was still 2 per cent. below its final value at the end of the first month.

The sample of radium enclosed in the small glass bulb, and fitted with a capillary tube handle, afforded a very convenient means of testing various types of cup radio-balance, and investigating the conditions of sensitiveness. All the cup balances made were tested in this way at various times. Although they differed greatly in sensitiveness and in details of construction, and some were quicker and more accurate than others, they all agreed within 0·5 per cent. in the values found for the radium on any given date. With the more accurate and sensitive balances, the values were consistent to 1 in 1,000, which is about the limit of accuracy in the determination of the thermoelectric power of the couples employed. The above example of observations taken with one of the most sensitive radio-balances, will serve to illustrate the order of accuracy attainable.

In taking readings, the radium bulb was placed in one cup (after it had been reduced to the temperature of the radio-balance by being allowed to remain for five minutes in a special cup provided for the purpose inside the copper cylinder), and a precisely similar glass dummy bulb similarly treated, was

Radio-Balance E. 7-ohm Galvanometer. September 20th, 1910. *Radium* 1·80 *mgm.*

—	Radium deflection. Ra and dummy bulb.		Balance method. Ra and dummy bulb.		Current deflection. Two dummy bulbs.		
Ra in cup ...	(1)	(2)	(1)	(2)	(2)	(1)	(2)
Temp. C. .—	15·20°	15·22°	15·26°	15·28°	15·30°	15·33°	15·36°
Current m.a..	—	—	−8·035	+8·032	−8·030	+8·029	−8·028
Deflection ...	+200·2	−202·7	+10·5	−13·3	−190·3	+188·8	−190·3
Results	Ra = 402·9 sc. = 255·4mw.		Ra = 240·3mw. + 23·8sc. = 255·4mw.		240·3mw. = 379·1sc. 1·5775 sc./mw.		

placed in the other cup, so as to balance the external conditions. Owing to the thermal capacity of the glass bulbs and their low thermal conductivity, the deflection took three or four minutes to become steady, and readings were taken at five-minute intervals. The radium and dummy bulbs were interchanged, and the opposite deflection taken. In the balance method, the current was reversed simultaneously with the interchange of the bulbs. In observing the deflection with the current alone, the radium was replaced by a dummy bulb, in order to compensate the loss of heat by conduction along the capillary glass handle. Under these conditions the deflection method always gave results agreeing very closely with the balance method, unless the temperature was varying rapidly, in which case the balance method was appreciably more accurate.

14. *Absolute Value of the Heat Emission of Radium.*

Although the relative values of the heat emission of samples of radio-active materials contained in similar bulbs may be compared with considerable accuracy by means of the radio-balance, the absolute values cannot be determined with the same degree of precision. The absolute values are dependent on comparisons made with the electroscope, the accuracy of which depends on the behaviour of the electroscope, and on the purity and constancy of the standards of comparison. The absolute value of the heat emission as measured by the radio-balance also requires correction for the fact that the radium bromide is enclosed in a glass bulb of low thermal conductivity, and is not in metallic connection with the copper cup like the peltier couples. More heat is lost by conduction along the capillary glass stem from the radium than from the copper cup or the peltier couples. The latter source of error might, no doubt, have been avoided by enclosing the radium completely in the cup, but this would have increased the difficulties of manipulation so greatly that the accuracy of the relative comparisons would have been seriously diminished. It was therefore considered preferable to determine this source of error and apply a correction. This was done by enclosing a standard thermo-couple with its two junctions in oil in a pair of glass bulbs with capillary tubes similar to the radium and dummy bulbs, and observing the current through the standard couple under these conditions required to balance a current through the peltier couples equivalent to the radium. In this way the various resistances to the flow of heat, and the losses by conduction along the capillary glass stems of the various bulbs, could be determined exactly as in an analogous electrical problem. The losses by conduction along the leading wires of the various couples and resistance coils employed in testing and calibrating the radio-balances were similarly determined. A similar source of error affects all comparisons in which it is attempted to compensate the heat emission of radium by heat generated in a resistance coil or couple with connecting wires. Angström employed for this purpose a resistance coil of only 0·365 ohm with a current of 140 milliamperes, and does not appear to have applied any correction for the heat generated in the leading wires, or for the loss of heat by conduction, neither of which were compensated. These effects must have been relatively large, and could only compensate each other by accident. With a 10-ohm coil and a current of 3 milliamperes, the errors were found to be quite appreciable with the radio-balance, and were in all cases carefully measured or compensated.

The loss of heat by conduction from the copper cups along the capillary glass stems of the dummy or empty bulbs, was easily measured by taking observations of the deflection due to a current through the peltier couples with and without the bulbs in place. The conductance could be expressed in microwatts per degree of temperature, or more conveniently in microwatts per scale division deflection. The resistance to the escape of heat, or the reciprocal of the conductance, is similarly expressed in scale divisions per microwatt. The resistance to the escape of heat from the copper cup by itself is denoted by Q. The resistance to the escape from the copper cup along the capillary glass stem is the sum of the resistance R between the copper cup and the glass bulb and the resistance S between the glass stem and the enclosing walls of the cylinder. The resistance to the escape of heat from the bulb itself along the glass stem is S alone, but the resistance through the copper cup is Q+R. In the first case, Q is in parallel with R+S, in the second Q+R is in parallel with S. The supply of heat to the inside of the glass bulb required to produce the same rise of temperature in the pile (or the same heat flow through Q) as heat directly communicated to the cup by the peltier couples, must be greater than the latter in the proportion of R+S to S. The correction was slightly different for the various bulbs, but lay between the limits 6 and 7 per cent. For the radium bulb it was 6·8 per cent. The heat supply 255·4 microwatts to the peltier couples required to balance the radium, must be increased in this proportion, giving 272·7 microwatts as the actual heat emission of the radium bulb.

The quantity of radium transferred from the original tube to the small bulb was determined by estimating the quantity remaining in the tube by means of the electroscope. The remainder was found to be 9·5 per cent. of the original 1·99 milligrams. Assuming none of the radium to have been lost in the transfer, this would make the contents of the bulb equivalent to 1·80 milligrams of radium, or 3·08 of $RaBr_2$. The rate of heat emission finally attained would therefore be 0·1515 watt per gramme of radium, or 130·5 gramme calories per hour. This is a minimum estimate, but includes most of the energy of the β-rays. The most uncertain factors in the calculation appear to be the exact radium content of the sample, and the effect of age on the heat emission. Generally speaking, all the phenomena observed agreed very fairly with the account given by Prof. Rutherford in his investigation of the heat emission of radium and its emanation (*Phil. Mag.*, February, 1904), but some facts were observed which appear to require further investigation, and it is possible that more accurate comparisons of the radio-activity and the heat emission may throw further light on some of the problems of the disintegration theory.

ABSTRACT.

In this apparatus, which was first constructed in 1905, heat supplied by radiation is directly compensated by the peltier absorption of heat in a thermo-junction through which a measured electric current is passed. In the simplest form of the instrument, radiation admitted through a measured aperture 2 mm. diameter falls on a small copper disc 3 mm. diameter by 0·5 mm. thick, to which two thermo-junctions

are attached, forming a peltier cross. One couple is connected to a sensitive galvanometer for indicating changes of temperature. The other is connected to a battery and rheostat in series with a milliammeter or potentiometer for measuring the current required to reduce the deflection of the galvanometer to zero. If A is the area of the aperture in square centimetres, H the intensity of the radiation in watts per square centimetre, a the absorption coefficient of the surface of the disc, P the peltier coefficient in volts, C the balancing current in amperes, and R the effective resistance of the couple, the equation giving the value of the radiation in absolute measure is

$$aAH = PC - C^2R.$$

The absolute value of P is the product of the absolute temperature by the thermoelectric power. The value of R, in the small correction term for the joule effect, is readily determined by observing the neutral current, $C_n = P/R$, for which the joule effect balances the peltier effect. In practice two similar discs with similar connections are mounted side by side in a thick copper box, and are balanced against each other in order to avoid changes of zero due to exposure to sunshine, or rapid variations of temperature.

The advantages of the disc radio-balance are that it is very simple to construct and easy to reproduce without material variation in the reduction constants. It is very suitable for measurements of solar radiation, or strong sources, but is insufficiently sensitive for weak sources; and the absorption coefficient a must be determined by comparison with a standard.

In the cup radio-balance the radiation is received in a copper cup 3 mm. diameter by 10 mm. deep, so that the absorption coefficient is practically equal to unity. Greater sensitiveness is secured by employing a pile of several couples, insulated from the cup, in place of the single balancing couple. External disturbances are eliminated by employing a pair of cups, similarly mounted but oppositely connected, enclosed in a thick copper cylinder. The joule effect, represented by the C^2R term in the equation, is automatically eliminated by passing the same current in series through the opposing peltier junctions soldered to the bottoms of the cups. The cup exposed to radiation is cooled, and the cup screened from radiation is heated by the peltier effect, while both are equally heated by the joule effect. A complete observation involves reversing the current and switching over the radiation screen, in order to eliminate any difference of sensitiveness of the two piles. By observing the neutral current, each cup can be used separately, as with the disc balance; but the disc balance cannot be used with the peltier couples connected in opposition, unless the balancing couples are insulated from the discs. The cup radio-balance is sensitive to less than a tenth of a microwatt, and is very suitable for measuring the heat evolved by small quantities of radio-active substances. It was applied to radium at Prof. Strutt's suggestion, and Prof. Rutherford very kindly supplied samples of emanation, and determined the value of the radium sample employed by comparison with his own standards. The second sample of emanation had only just come to hand, and the absolute values had not been finally reduced at the time the paper was read; but it appeared from the preliminary reductions that the heat evolution of radium in terms of Prof. Rutherford's standards was somewhat greater than that given by previous observers.

The instruments exhibited at the meeting included (1) a disc radio-balance equatorially mounted for solar radiation; (2) a laboratory pattern, with water or steam jackets for determining the temperature coefficients and for investigating the theory of the instrument; (3) a cup radio-balance, with which the heat production of radium and its emanation were demonstrated. The latter intrument is capable of measuring the heat evolved from 1 milligram of radium to about one part in 1,000.

DISCUSSION.

Dr. J. A. **Harker** expressed his interest in the paper, especially in the details of the construction of the various instruments exhibited.

Mr. A. **Campbell** asked what were the materials of the wires used for the thermo-couples.

The **President**, in reply to Mr. Campbell, said he had used iron-constantan couples.

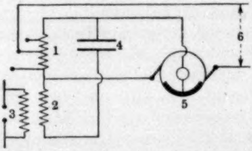
II. *Demonstration of a New Method for producing High-tension Discharges. By* Prof. ERNEST WILSON *and* Mr. W. H. WILSON.

OCTOBER 28TH, 1910.

[ABSTRACT.]

ACCORDING to this method, energy is taken from an alternating or continuous-current source and stored in a magnetic field by an inductance; it is then permitted to surge into a condenser which forms with the inductance a *low* frequency oscillating circuit. When the energy is stored in the condenser the latter is mechanically bridged across the primary winding of a spark-coil, with which it forms a *high* frequency oscillatory circuit. The energy is then transmitted by the secondary winding of the spark-coil to the work circuit in a well-known manner.

The method, therefore, involves the use of an inductance, a condenser, a spark-coil and a special contact-making device.



1 = **Inductance.**
2 = **Primary winding.**
3 = **Secondary winding.**
4 = **Condenser.**
5 = **Contact maker.**
6 = **Supply mains.**

In one form the contact maker consists of a cylinder of insulating material driven by a small motor, the cylinder carrying a metal segment, and a complete ring of metal attached to the segment. Three brushes (say, A, B, C) bear upon the cylinder. A makes contact with the segment once in a revolution, B is continuously in contact with the ring, and C makes contact with the segment once in a revolution, and is mounted on an adjustable arm. According to one method of connection, the coil of the inductance is connected in series with the primary winding of the spark-coil and forms one circuit. In parallel with this circuit is placed the condenser. One pole of the supply source is connected to brush A on the contact maker. The other pole of supply is connected to a tapping point on the inductance suitable for the voltage of supply, the total number of turns on the inductance depending upon the voltage at which it is desired to charge the condenser. The junction of the inductance and the condenser is connected to brush B on the contact maker, and is, therefore, continuously in contact with the metallic segment. The junction of the inductance and the primary winding of the spark-coil is connected to brush C, which is capable of adjustment.

At the epoch of "make," the brushes A and B are connected by the segment, and the system is switched on to the supply source. The current rises in the winding of the inductance and builds up the magnetic field. The condenser also becomes charged, and its charging current passes through the primary winding of the spark-coil. At the period of "break," brush A severs contact with the segment and is insulated. The energy stored in the condenser is added to that in the inductance, and the condenser begins to be charged in the reverse direction. Ultimately the whole energy in the system is stored in the condenser. Brush C is so adjusted that, at this moment, it begins to touch the segments of the contact maker, and thereby short-circuits the inductance. The condenser then discharges with great rapidity through the primary winding of the spark-coil, and the energy oscillates to and fro. As is well known, the E.M.F. induced in the secondary winding may be of an oscillatory or unidirectional character, according to the nature of the work circuit. As only the current in the condenser passes through the primary winding of the spark-coil at "make," the inverse E.M.F. induced in the secondary winding is so small as to be negligible, but if desired it can be entirely eliminated by a modification in the connections.

Briefly, the following are some of the advantages gained :—

1. For X-ray work the inverse E.M.F. at "make" is eliminated, thereby leading to increased life of the tube and to a more sharply defined radiograph.

2. Only a small magnetising current is required as the inductance has a nearly closed magnetic circuit. This gives rise to a very small C^2R loss, and consequently higher efficiency.

3. On account of the long periodic time of the system between the periods of "break" and "short-circuit," the voltage across the contact of the interrupter at "break" does not rise to a high value, or rises so slowly that the contacts are well separated before it is developed. Hence immunity from sparking.

4. The method lends itself to few secondary turns, and this keeps down the time constant. It also makes the coil lighter, cheaper and more compact.

5. The method lends itself to low secondary resistance—a point of great importance in connection with radio-telegraphy.

6. The iron of the spark-coil can be kept small in amount, and special attention can be paid to its lamination and insulation, as it may have to be operated at frequencies of three or four thousand per second.

7. The elimination of sparking increases efficiency, and on board ship, where coal gas is not conveniently obtainable, this is an advantage.

8. The apparatus can be worked from direct-current or alternating-current systems at usual voltages, or from a portable battery of a few storage cells.

9. The oscillatory current at " break " does not pass through the battery, and hence does not assist in its discharge.

10. The iron in the magnetic circuit of the external inductance has only to operate at low frequency, and hence it has not to be finely laminated.

11. When used on alternating-current systems rectification, if desired, is easily effected by employing two short-circuiting brushes, one for each half period, and allowing the second brush to short-circuit at the moment when the condenser is fully charged after allowing a second complete surge of the energy between the condenser and the inductance of the spark-coil.

12. The apparatus is light, efficient and cheap, and is suitable for radio-telegraphy, X-ray, medical and other work in which high-tension electricity is employed.

The following are a few particulars of the apparatus exhibited at the meeting :—

Spark length of secondary winding	10 in.
Secondary turns	16,000 to 20,000
Primary turns	78
Secondary resistance (aluminium)	2,900 ohms
Primary resistance	0·056 ohm
Weight of spark coil and condenser	10 lb.
Weight of inductance and interrupter	20 lb.

DISCUSSION.

Mr. A. Campbell asked if the adjustment of the commutator was made experimentally, and if the instrument could produce high-frequency oscillations suitable for wireless telegraphy.

Mr. W. **Duddell** asked if the discharge through the X-ray bulb in the experiment shown at the meeting was unidirectional.

Prof. J. A. **Fleming** asked if the coil could be used for medical work. He also asked if the frequency of the discharge could be made high enough for wireless telegraphy, and commented on the small amount of sparking at the commutator.

Dr. R. S. **Willows** suggested that the nature of the discharge in the secondary could be easily determined by using a vacuum tube rectifier.

Mr. **Addenbrooke** asked how much energy could be obtained from the secondary of the coil exhibited.

Prof. **Wilson**, replying, said the adjustment was performed experimentally. Using an air coil for primary, enormous frequencies could be obtained in the secondary. The efficiency of the instrument was high compared with that of ordinary coils. The E.M.F. induced in the secondary on open circuit was oscillatory, but when loaded the damping could be such as to cause the E.M.F. to be unidirectional. In the experiment shown with an X-ray bulb the discharge was nearly unidirectional.

III. *The Behaviour of Steel under Combined Static Stress and Shock.* *By* F. ROGERS.

RECEIVED AUGUST 30, 1910. READ OCTOBER 28, 1910.

THE behaviour of materials, particularly steel, under rapidly applied stress—equally vaguely described as "shock"—is probably at once the most difficult and the most important branch of elasticity which yet remains to be fully investigated.

Investigators who have approached the problem from the point of view of determining what steels possess in the highest degree the useful property of offering a great resistance to rupture under a blow, or detecting a lack of this property, seem generally to have had to content themselves by choosing a convenient "velocity of tup" or "speed of shock," which has been determined largely by the limitations of the apparatus and type of test sample selected. No information as to the stresses set up by the blow in tests carried out in these machines is obtainable; and, of course, up to a certain point, such information is not necessary.

Under this category we have the machines of the pendulum class, such as those of Charpy and Izod, the rotating toothed disc of Guillery and the falling weight of Frémont, in which notched bending test-pieces are used, so that the determination of the stresses due to the blow is impossible.

In the falling-weight machines of the types designed by Hatt, and by Stanton and Bairstow, the great advantage of having direct, instead of bending, stresses is achieved, but in the present forms the determination of stresses would be attended with uncertainty, and appears not to have been attempted.

On the other hand, considerable attention has been devoted to the mathematical study of the propagation of stress in a perfectly elastic body upon which another impinges, and in the past few years the mathematical theory, apparently first approached by Newton, has been experimentally verified by J. and B. Hopkinson,* Sears † and others. The mathematical theory is based upon conditions and assumptions which have as result that each section successively of a rod which receives a longitudinal blow at one end is *instantaneously* jerked to a maximum stress which gradually dies away. Graphically, the wave of stress, with an absolutely steep front, advances along the rod with the velocity of sound in the material of the rod.

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* Royal Soc., Proceedings, Series A, 1905, pp. 498-506.

† Cambridge Phil. Soc., Trans. 21, No. 2, pp. 49-105, August 25, 1908.

Between this extreme, and infinitely slow (" static ") loading, all gradations of the rate of increase of stress do occur—*i.e.*, it is possible to have any slope of wave front ; this slope is the rate of increase of stress. The definite time lag in the growth of the stress to a maximum value is the same for all rates of loading in the same material, and with the same kind of stress ; in other words, the rate of translation of the wave of stress is independent of its shape. Thus, when a longitudinal pull, push or blow is applied at one end of a bar of steel about 3½ miles long it is always one second later when the same forces exactly are felt at a point towards the other end of the bar. The decay of these forces and their subsequent complication by reflection need not be considered here.

In addition to affording some confirmation of the mathematical theory of the propagation of stress, B. Hopkinson's experimental investigation just quoted, in which a weight was dropped upon a stop attached to the lower end of a long hanging wire, showed that if the stress exceeded the elastic limit for 1/1000 second or less it might exceed even the so-called static ultimate stress without the occurrence of rupture or permanent deformation of the wire. In tests by this method rupture was unaccompanied by any considerable permanent extension of the wire.

The fundamental physical importance of the rate of increase of stress at a given point or section in a piece of material has, however, received scant recognition. It is precisely this variable, *the time rate of increase of stress ds/dt*, which it appears to me makes all the difference when we consider the behaviour of a metal which is capable of elongation under " static " loading. This seems to have been, somewhat indistinctly perhaps, the quest of those workers who from time to time have made tests of steel at various " speeds of loading " within the range obtainable by means of ordinary tensile testing machines, and who have generally concluded that within the usual speeds the variation of properties is only slight. Again, shock tests have been made with various " speeds of tup " (sometimes called " speed of shock " or " velocity of shock ") ; or keeping the striking velocity and energy of the tup the same for each test, the sharpness of the notch has been varied.

Despite the occasionally conflicting nature of the evidence it suggests that, in a general sense, as the *rate of increase of* *stress* is increased the nature of the breakdown is essentially altered. The deformation sustained by the material becomes less, the distribution of the deformation is altered, and usually the energy absorbed in rupturing the piece becomes less.* That there is an essential difference in the intimate nature of the deformation is shown by the microscopical investigations of Osmond, and of Stanton and Bairstow, and others. There is doubtless a progressive change as the rate of increase of stress becomes greater. At low rates of increase of stress the deformation occurs principally by crystalline cleavages, recognised as slip-bands, and at high rates Neumann lamellæ are formed. This is illustrated to a certain extent by the difference often noticed between the fracture of a piece which has " broken short under a blow " and an ordinary steady tensile or bending test fracture, especially, *e.g.*, in the case of common bar iron.

The exact determination of the rate of increase of stress will admittedly present much difficulty whenever it amounts to what is commonly designated " shock." In a few simple cases, it is true, the rate of growth of stress within the elastic limit can be directly calculated ; but this is not of much help, as our final appeal will probably be rather to measurements of the energy required to produce fracture of a specimen than to measurements of strains. In the simplest method of determining the energy required to rupture a specimen—viz., bending tests of notched pieces—the calculation of stresses becomes extremely difficult, owing to the effects upon their distribution due to (*a*) the fact that the specimen is a beam under central load, (*b*) the fact that the specimen is notched, and (*c*) the rate at which the load is applied ; and owing to the difficulties of recording the relation of time and the pressure between the tup and the test-piece.

Nevertheless, several more or less indirect methods can be devised, and it is now proposed to record the results of some preliminary experiments made in one such way. It is remarkable that this indirect method simultaneously yields additional intrinsically valuable information.

In my experiments, specimens of steel were tested by shock whilst supporting a steady load. Keeping other factors constant, a high rate of increase of stress is thus attained when the blow is applied. The question whether the behaviour of steel under shock depends (apart from the reason of any dependence) upon the value of any initial static stress in the steel appeared in itself to be sufficiently important to warrant investigation, for static stress and shock act frequently in combination in structures.

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* In order to avoid cumbrous circumlocution, a sufficient use of " ceteris paribus " is taken for granted.

The tests were made by means of an apparatus whose cost was borne by the Government Grant Committee of the Royal Society. Its peculiarity of design is in part due to the fact that it was contemplated to use it for several experimental methods of approaching the general problem of the effect of rate of increase of stress. It is a shock testing machine of the pendulum type. A general view is given in Fig. 1. Not only, however, is the hammer (weight 60 lb.) suspended, but the anvil (weight 260 lb.) also forms another pendulum, the two having a common axis of suspension and common radius of gyration (3·80 ft.). The specimen rests on two supports at the centre of percussion of the anvil, and the striking point of the hammer is at the centre of percussion of the hammer. The movements of the pendulums are indicated by two pointers carried over a dial. From the movements the energies are calculated.

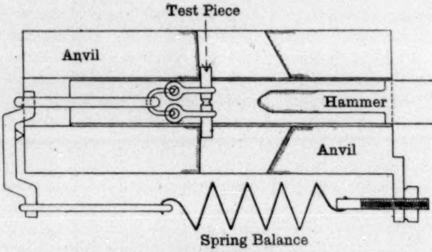

FIG. 3.—DIAGRAMMATIC SECTION.

The maximum energy of blow available is 440 ft.-lb.*

As represented in the photograph, Fig. 2, and diagrammatically in Fig. 3, the static bending load up to 800 lb. is applied,

* The only other shock testing machines of which I have been able to find any record in which both hammer and anvil are free to move are (1) that of F. Kick, which was used for shock compression tests only ("Das Gesetz der proportionalen Widerstände," 1885; Leipzig, Arthur Felix), and (2) one used at the National Physical Laboratory, but which I believe is not at present in use (Inst. Mech. Engineers, Proc., A., pp. 857-959, October-December, 1905).

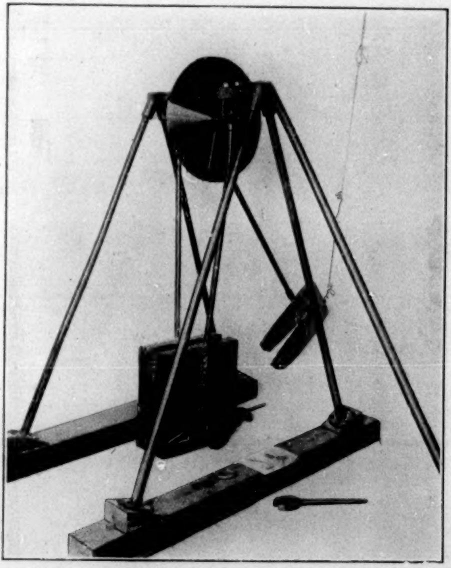

Fig. 1.

To face p. 42.

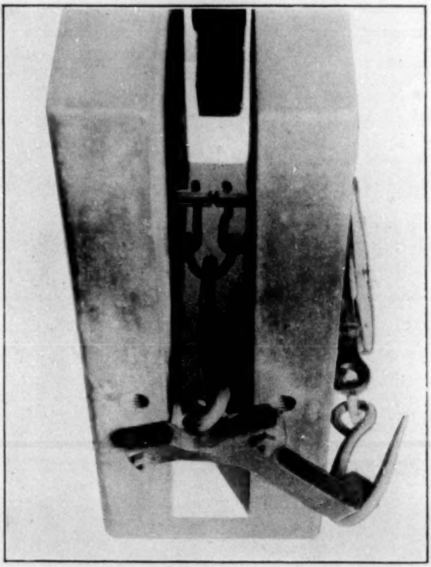

Fig. 2.

in the same direction as the blow is to be applied, at two points on the specimen equi-distant from its centre by means of a light combination bearing upon the anvil and consisting of a nut, screw, lever, system of links and spring balance. The links are arranged to leave the centre of the specimen clear to receive the nose of the hammer. When the hammer strikes the specimen the static pull is released naturally in consequence. A spring was selected for the static loading in preference to dead weights in order to avoid inertia effects and dynamic losses.

A rough calculation shows that, with the gear and striking velocities adopted, when the test piece is struck the static pull is released in a negligibly short distance, and is not caught up again.

Results of Tests.

Table I. gives the results of a series of experiments upon test pieces of a mild steel of the following percentage composition :—

Steel A.

C.	Mn.	Si.	P.	S.
0·16	0·45	0·008	0·019	0·027

Bars ½ in. diameter, reeled.

Table I.

Diameter of test pieces	0·500 in.
Diameter at bottom of groove	0·240 in.
Span	2·18 in.
Striking energy of hammer	230 ft.-lb.
Striking velocity of hammer	15·6 ft. per second

The sides of the groove were inclined at 60 deg. to one another and joined by a curve of about $\frac{1}{50}$in. radius. All were formed by the same tool.

"Calculated extreme static stress." Tons per square inch. (*See* explanation below.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	35·27	0·20	All from one bar
21·0	32·75	0·25	
40·5	31·23	0·28	
56·7	21·96	0·09	
64·8	20·79	0·07	
72·9	24·73	0·10	From another bar of the same lot of steel
80·6	24·71	0·12	

What has been called, for want of a better term, the "calculated extreme static stress" in the first column, is the greatest stress which would be produced in a beam of uniform circular cross-section having the same diameter as the test piece has at the bottom of the groove and with the same static loading, assuming the material of the beam to be perfectly elastic up to such a stress. The value is thus directly proportional to the load, but not necessarily to the actual stress existing in the piece, especially at the higher loads, when it does not lend itself to calculation, but is certainly less than the value given.

These results are plotted in Fig. 4, curve A.

They indicate that as the static stress is increased there is a very considerable falling off in the energy absorbed in rupturing the steel by shock, amounting at the highest loads to about 30 per cent.

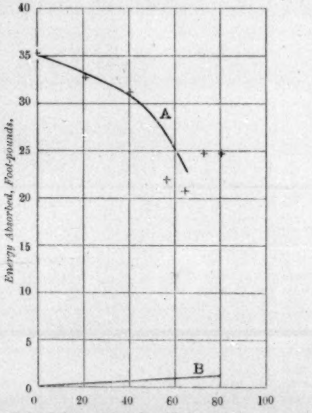

FIG. 4.

Curve A.—Energy absorbed in rupturing test piece by shock.
Curve B.—Work done on test piece by the static load.

It is important to notice that this falling off is in all cases much greater than the work which the "static" load does upon the test piece. An approximate representation of the work done by the static load is given by curve B, which is calculated from observed deflections. With the highest static load this is only about 5 per cent. of the energy required to rupture the piece by shock alone under the conditions adopted.

A few further results will now be given for tests in which one or more of the conditions was varied from those adopted in the above series. In each of these experiments two identical test-pieces were tested with a high static stress and with little or no static stress respectively, as recorded below.

Table II.

All test pieces were of 0·500 in. diameter and 2·18 in. span. The groove was of the same form for all as described in Table I. The static loading arrangement was the same for all, and was in place upon the specimen whether any static load was applied or not.

Steel A.—Composition as above.

Diameter at bottom of groove	0·224 in.
Striking energy of hammer	230 ft.-lb.
Striking velocity of hammer	15·6 ft. per second

"Calculated extreme static stress." Tons per square inch. (*See* explanation above.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	26·94	0·08	...
71·6	19·41	0·08	Very slight permanent set perceptible under the static load

The observation in the last column indicates that the elastic limit had been reached at a considerably lower load than was used.

Steel B.—Composition per cent.

C.	Mn.	Si.	P.	S.
0·45	0·55	0·093	0·017	0·030

Bars ½ in. diameter, reeled.

Diameter at bottom of groove. Inches.	"Calculated extreme static stress." Tons per sq. in. (*See* explanation p. 43.)	Striking energy of hammer. Foot-pounds.	Energy absorbed in rupturing test piece by shock corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0·230	4·5	230	16·99	0·04	...
	92·0		9·81	0·02	...
0·170	0	77	7·65	0·03	A similar test piece gave an appreciable permanent set at 95·6 tons per sq. in. "calculated extreme static stress"
	95·6		5·48	0·02	

Steel C.

Carbon about 0·25 per cent.

Bars $\frac{5}{8}$ in. diameter, reeled.

Diameter at bottom of groove .... 0·200 in.
Striking energy of hammer .... 77 ft.-lb.
Striking velocity of hammer .... 5·2 ft. per second

"Calculated extreme static stress." Tons per square inch. (*See* explanation above.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	9·52	0·03	A similar test piece gave an appreciable permanent set at 92·2 tons per square inch "calculated extreme static stress"
92·2	5·48	0·02	

These results point to the same conclusion as those in Table I.—viz., that much less energy is absorbed in rupturing specimens by shock if they be under static stress than if they be initially unstressed.

It is interesting to note incidentally that the energy transmitted to the anvil is almost negligibly small in all the tests recorded here.

CONCLUSIONS.

The important conclusion that *steel is substantially less resistant to shock whilst it is under static stress* appears to be definitely established in a preliminary fashion by these experiments. It is noteworthy that the correction for the work done in applying the static stress is small; that is to say, that *the energy absorbed in breaking a sample of steel is greater when entirely applied as shock than when applied partly as shock and partly statically.*

The explanation of this difference is chiefly, I think, the difference in the rate of increase of stress at the higher stresses in the two cases. The actual values of the highest stresses which occur are not necessarily identical. In the light of B. Hopkinson's result (already quoted, *see* p. 40) and the present experiments, I would infer that, for a given steel, the higher the rate of increase of stress the higher is the stress reached before rupture occurs. At the same time, as already mentioned (*see* p. 41), the total deformation of the piece, speaking generally, becomes less, and the intimate nature of the breakdown may also be expected to show a corresponding variation.

It is possible that there may be an additional cause of the loss of toughness of steel occasioned whilst it is under static stress, which depends directly upon the nature of the intermolecular forces. In the present state of knowledge, however, such an explanation would be highly conjectural, and is therefore passed over.

At the higher static stresses employed, test pieces were perceptibly strained beyond their elastic limits, and this may also help to account for a part of the diminution in the resistance to shock.

The author is inclined to attach some, but relatively little, importance to this factor. It must not be inferred from this, however, that I am prepared to admit that a piece of steel slightly overstrained statically would *upon removal of the static load* be any more brittle under shock than if it had not been so strained. The contrary is indeed sometimes the case.

The investigation has not been carried far enough to show the precise relation between the energy absorbed in rupturing and the rate of increase of stress. Experience would lead one to expect that the relation would be different for different steels—that the energy absorbed decreases comparatively rapidly as the rate of increase of stress rises in the case of steels alleged to have given good tensile test results but poor resistance to shock.

Attention has been drawn by the writer * to the possibility that considerable internal stresses arise in steels during cooling, due to the difference between the coefficients of expansion of ferrite and pearlite. As these stresses are to be expected to depend upon many factors, as, for example, the size and shape of the individual constituent grains, they will doubtless vary considerably from point to point throughout a bar of steel, and would probably not be eliminated by annealing. In the light of the present results, this affords a new partial explanation of the variations which investigators have at times experienced in the results of shock tests of samples cut from the same piece of steel, for the resistance of steel to shock is very sensibly dependent upon the value of any static stress which exists in it.

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* **Iron and Steel Institute Journal, 1905, 1, p. 489.**

The writer thanks Messrs. Edgar Allen & Co. and Messrs. Thos. Andrews & Co. for some of the samples of steels which he has employed in this investigation.

ABSTRACT.

Attention is drawn to the importance of the time rate of increase of stress, *ds/dt*, in the behaviour of materials under stress. The exact determination of this rate must usually present much difficulty, but the indirect experimental method adopted consisted in submitting specimens of steel to shock whilst under static loading. The machine used for this purpose was exhibited and described at the meeting, and contained some novel features. The important conclusion that steel is substantially less resistant to shock whilst it is under static stress appears to be definitely established by these experiments. In some cases the effect of a large static stress was to diminish the resistance to shock by as much as 30 per cent.

It is noteworthy that the correction for the work done upon the sample in applying the static load is relatively small. Thus, the energy absorbed in breaking a sample of steel is greater when entirely applied as shock than when applied partly as shock and partly statically. This difference is considered to be due chiefly to the difference in the rate of increase of stress at the higher stresses in the two cases. The actual values of the highest stresses are not necessarily identical, but probably the higher the rate of increase of stress the higher is the greatest stress reached before rupture occurs; whilst simultaneously deformation is diminished, and the intimate nature of the breakdown suffers a corresponding variation. An explanation depending directly upon the nature of intermolecular forces is so conjectural in the present state of knowledge that it is not pressed. At the higher static loads employed some portions of the test pieces were stressed beyond their elastic limits, and this may also help to account for a part of the diminution in resistance to shock—a factor to which the author is, however, inclined to attach comparatively little weight, because a piece of steel which has been overstrained statically is not necessarily thereby made more brittle. The importance of the effect of internal stresses in steel upon its resistance to shock is emphasised in view of the present results.

DISCUSSION.

Mr. R. S. **Whipple** asked if the same results were obtained with high and low carbon steels, and whether the results were affected by annealing.

Mr. **Etchells** asked what the author meant by "calculated extreme static stress," and also how the stress was applied. It was difficult to calculate the stress in the author's experiments.

Prof. **Smith** said the paper opened up a field for research, and then described some experiments which he had been making recently on the time rate of change of strain.

The **Author**, in reply to Mr. Whipple, said he had obtained the same results from steels with carbon varying from 0·16 to 0·45 per cent. The actual apparatus was exhibited and the manner in which the stress was applied was shown.

IV. *On the supposed propagation of "Equatorial" Magnetic Disturbances with velocities of the order of* 100 *miles per second.* *By* C. CHREE, *Sc.D., LL.D., F.R.S.*

RECEIVED OCTOBER 14, 1910. READ NOVEMBER 11, 1910.

IN a recent number of "Terrestrial Magnetism,"* the question of the simultaneity of magnetic disturbances recorded at different stations has been discussed by Dr. L. A. Bauer † and Mr. R. L. Faris.‡ A good many magnetic storms have so-called "sudden commencements," an outstandingly rapid change of force occurring, often without any previous indication of disturbance. As regards these "sudden" changes, three things are conceivable: They may be absolutely simultaneous at different stations; there may be a very small difference of time, corresponding to the rate of propagation of electromagnetic waves; or, finally, there may be, as Dr. Bauer concludes, longer intervals, amounting to several minutes, for stations remote from one another. Under existing conditions of registration one cannot decide between the first two possibilities. As between these two and the third a decision should not be impossible. The best, if not the only, way of settling the question is by reference to actual records, but there are a variety of general considerations which should be borne in mind in any such comparison.

If a magnetic storm is due to any form of electric current, whether overhead or underground, the commencing disturbing force is unlikely—except accidentally and on rare occasions—to be wholly in or wholly perpendicular to the local magnetic meridian, or wholly along or wholly perpendicular to the local vertical. It would, for instance, be an extraordinary kind of external disturbing force which commenced by changing the declination and nothing else all over the world. Thus it is probable that, except on rare occasions, sudden commencements represent forces having components in the directions alike of D, H and V (Declination, Horizontal Force and Vertical Force). This is quite consistent with the disturbance being visible earlier in the trace of one element than in those of the others. These "sudden" commencements represent, at least as a rule, a force gradually increasing from zero, and the exact time at which the movement becomes apparent in a magnetogram depends on the sensitiveness of the particular instrument—in instruments of the same type mainly on the openness of ordinate scale—and on how nearly the disturbing force is perpendicular to the magnet's axis.

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* Vol. XV., No. 2, 1910.

† *L.c.*, p. 107.

‡ *L.c.*, p. 93.

Mr. Faris' figures from the U.S. Coast and Geodetic Survey stations show a decided tendency for commencing movements to be earliest visible in the H trace and latest in the V (Mr. Faris' Z). This probably means only that in most of the cases considered the disturbing force had a larger component in than perpendicular to the magnetic meridian—a phenomenon * already noted at a variety of stations—or that the change of force answering to 1 mm. of ordinate was less for the H than for the D trace. The fact that the V commencement tended to be the latest arose probably in part from the fact that the commencing vertical force is usually considerably less * than that in the horizontal plane, but not improbably in part from a slight frictional opposition to movement in the vertical force magnets.

If circumstances were absolutely alike at all stations, then Mr. Faris' plan of deriving the interval between commencements at different stations by comparing the D, H and V records separately would be more satisfactory than it actually is. When, however, we compare two distant stations—especially when their latitudes are so different as those of Sitka and Porto Rico, two of the Coast and Geodetic Survey stations—the disturbing forces are likely to differ considerably in amplitude and in orientation relative to the local magnetic meridian and vertical. If at both places the H component predominates and is large, and the two H instruments are equally sensitive, then a comparison of the commencing times shown by the two H traces may be the best way of measuring the apparent difference in time; but it obviously may not be so if the H component at one station is much smaller than at the other. This probably partly explains the large differences between the results which Mr. Faris obtains in the case of the same storm from the D, H and V curves treated separately. For instance, the time for propagation of a disturbance completely round the earth which he derives from his example No. 5 varies from 0·5 minute when V curves are considered to 11·6 minutes when H curves are considered.

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* National Antarctic Expedition, 1901-1904. Magnetic Observations. Chap. IX., especially p. 181.

Assuming the disturbing force finite and continuous, then, however large it may be, some time must elapse before the change in the curve ordinate is measurable. Thus all traces naturally post-date the commencement, and, apart from instrumental error, if we take the earliest time shown by *any* trace, whether D, H or V, we cannot ante-date the event. There is, of course, a risk of supposing two movements to correspond which are really due to different sources, but this risk, though possibly reduced, cannot be wholly avoided even if we confine our attention to one and the same element at the two stations.

A peculiarity of Dr. Bauer's theory requires to be mentioned here to explain Mr. Faris' method of procedure. It was suggested recently by Prof. Kr. Birkeland* that "sudden commencements" are due to an overhead current in the magnetic equator. Birkeland had observed that these movements are usually largest in the H trace, and he believed them to be normally largest in equatorial regions. At the same time, the amplitude was admitted by Birkeland to be considerable even in temperate latitudes, and to account for this he recognised that the hypothetical equatorial current must be at a great height above the ground, a height, in fact, comparable with the earth's radius. Dr. Bauer has examined Prof. Birkeland's theory and has come to the conclusion that it is not correct as regards the height of the current, and that while an overhead current is the true explanation, its height is only of the order of 100 km., and its plane is that of the geographical, not the magnetic, equator. I myself some time ago, when discussing the magnetic results of the National Antarctic Expedition † of 1901–4, pointed out that the "sudden commencements" apparent in the Antarctic curves seemed incompatible with the truth of Birkeland's theory. They seem, however, still less compatible with the truth of Bauer's theory. This theory, moreover, seems to me unsatisfactory in several respects, but this may be my fault, as I am unable to follow the mathematical calculations. Dr. Bauer seems to suppose an ion or group of ions suddenly to commence describing a circular orbit in the earth's equator, making a complete revolution in from three to four minutes, and apparently he supposes that all points on the same geographical meridian experience a commencing disturbance simultaneously. Presumably, the disturbance commences when the leading ion reaches the meridian in question. The E.M.F. which Dr. Bauer associates with the ionic movement seems to be that of the earth's potential gradient, and apparently he regards the field as having the same strength—100 volts per metre—as at the ground level. This I cannot follow at all, because a vertical field seems incapable of explaining a horizontal current, and, so far as we know, the earth's electric field practically disappears within a few miles of the earth's surface. Then even if some object, say a satellite, carrying an electric charge, went round the earth in the equatorial plane, completing a revolution in 3¾ minutes, one would expect electromagnetic waves to start from the satellite at every instant and to travel with a velocity equal or similar to that of light, spreading out in all directions, and not merely in the meridian plane containing the satellite at the moment. Thus some disturbance ought to be felt at every spot on the earth's surface practically as soon as the satellite's movement began, and one would not expect any marked maximum of disturbance when the satellite reached the meridian containing the station, unless the satellite passed nearly overhead. Thus I fail to see why, even if a satellite existed, a disturbance should travel round the earth in one direction only; and supposing such a peculiar form of propagation possible, there is no obvious reason why it should not circumnavigate the earth several times. The amplitude of the disturbance is often of the same order at stations such as Kew, Bombay and Christchurch, N.Z., so that a complete circuit of the earth ought, one would think, to be far from sufficient to reduce the disturbance to vanishing point. Thus what one would expect if a comparatively slow rate of transmission exists is a *succession* of wave-like movements, crests and hollows following one another at nearly regular intervals. This type of phenomenon is, as a matter of fact, not at all an unusual one, the interval between successive crests varying on different occasions from several minutes to a few seconds. But this is essentially *not* typical of "sudden" commencements, at least as recorded in temperate latitudes. At Kew, for instance, what one usually does see in the H trace is a rise lasting four or five minutes, sometimes preceded by a relatively insignificant fall lasting a minute or two. The rise seldom goes on for more than 10 minutes, but the element usually retains an enhanced value for at least 15 or 20 minutes, and usually there is no obvious repetition of the commencing movement.

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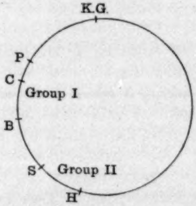
* The Norwegian Aurora Polaris Expedition, 1902–1903, Vol. I., pp. 78, *et seq.*

† *L.c.*, p. 274. *See* also "Nature," May 19, 1910.

To return to Mr. Faris' paper: following Dr. Bauer, he regards a disturbance as travelling *either* eastward or westward, appearing simultaneously at all points in the same meridian. We may thus regard his five stations P (Porto Rico), C (Cheltenham, U.S.), B (Baldwin), S (Sitka) and H (Honolulu) as situated on the same circle, at distances corresponding to their difference of longitude from Greenwich (G), with which we may regard Kew (K) as coincident. Mr. Faris splits his stations into two groups, P, C and B being included in Group I., and S and H in Group II. He takes a mean time of commencement for the Group I. stations and a mean time for the Group II. stations, and regards the difference between the two as representing the time taken by the disturbance to travel round the circle over the arc intervening between the two imaginary stations which represent the centres of the two groups.



If the disturbance be assumed to travel counter-clockwise—*i.e.*, to the west—and the time of commencement at Group I. is the earlier, then the disturbance must have originated somewhere in the long arc between H and P, and the Group II. time will follow the Group I. time by the time required to travel 68° (the short arc between the imaginary centres of the two groups). If, however, still supposing a westerly propagation, the Group II. time is the earlier, then the disturbance must be supposed to originate in the short arc between B and S, and the time interval between the imaginary central stations must represent that required to go through 360°–68°, or 292°. With disturbances assumed to be travelling eastward, the origin must be regarded as in the long arc between H and P, or in the short arc between B and S, according as the time interval between the imaginary central stations is supposed to answer to an arc of 68° or to one of 292°. As the long arc from H to P is 267°, while the short arc from B to S is only 40°, one would expect only a small minority of storms to originate in the short arc; it is thus somewhat surprising to find that the majority of Mr. Faris' storms—in fact, 9 out of 15—must be regarded as originating in the short arc, as they are supposed to take the long route in passing between the imaginary central stations. Another suggestive phenomenon is that the average of the times of completely encircling the earth, calculated from the differences of the times of commencement at the imaginary central stations, is according to Mr. Faris' own figures—taking the mean from his D, H and V results—only 2·9 minutes for the disturbances taking the long route between the central stations, as against 4·3 minutes for those taking the short route. Further, if instead of following Mr. Faris' method, we take the earliest time shown by any trace at a station as the true commencement for that station, the means from the disturbances taking the long and the short routes become respectively 2·8 and 4·9 minutes.

Mr. Faris' reason for splitting his stations into groups is the uncertainty in fixing the exact time of a disturbance on a particular trace. By taking a mean from several stations this uncertainty is reduced. A great deal turns on what this uncertainty actually is. Mr. Faris (*l. c.* p. 99), seems to claim that with "especial care"—*i.e.*, presumably with the care actually exercised in his investigation—times can be scaled to an accuracy "within one half-minute." This means, I think, only that the time at which the centre of the spot of light fell on a particular point of the photographic paper can be determined after the photograph is developed with an error not exceeding ½ minute. This, I may say in passing, is a high standard to maintain in records having the ordinary openness of time scale. The claim may be warranted if interpreted in this way, but not, I think, if the meaning intended is that the true time of commencement of a disturbance can be fixed to within ½ minute. As evidence to the contrary, we have the fact that the times of commencement deduced by Mr. Faris from his H and D curves differed on the average by +0·4 minute—the + sign denoting earlier occurrence in H—individual differences varying from −0·9 to +1·7 minutes, while the corresponding differences in the case of the H and V curves averaged no less than +2·3 minutes.

From the arguments advanced earlier, the difference between the times of commencement derived from the different traces cannot well represent anything but difference between the errors in the times assigned to the actual commencements. An examination of Mr. Faris' individual disturbances points to the same conclusion. In almost every case some of the results appear to be inconsistent with the view that the times given are true measurements of the commencements to within 0·5 minute. If they were so, then the interval between the times at any two stations should not differ from the interval deduced from the rate of propagation calculated by Mr. Faris by more than one minute. A few examples must suffice. Disturbance No. 3 is supposed to be going west, passing the Group II. stations earliest. Thus the time at P—see the figure—should precede that at C; on the contrary, the earliest record at C preceded that at P by 2·8 minutes. Disturbance No. 8 is supposed to be going west, passing first the Group I. stations. Mr. Faris' estimated times of encircling the earth for this disturbance vary from 2·1 to 9·5 minutes. The longitude interval from P to C is 11° 24′, so that the times of commencement should have differed by only 0·3 minute at the most, or, allowing uncertainties of ±0·5 minute, by 1·3 minutes. The actual difference of the earliest commencements given by Mr. Faris is 4·4 minutes. Finally, disturbance No. 10 is supposed to be travelling east from Group II. to Group I. stations. Thus the occurrence at S should precede that at B by the time taken to go 40°—the difference in longitude between S and B; on the contrary, the first record at B precedes that at S by 3·0 minutes.

The order of priority of occurrence at the five Coast and Geodetic stations shows some curious phenomena. When disturbance takes the long route in passing between Group I. and Group II. stations, B should come first or fifth, according as the disturbance travels east or west. As a matter of fact, it is once fifth, but six times second or fourth, and never once first. On the other hand, when the disturbance takes the short route B should always come third. It is, however, only once third, as against five times first or second. In deriving these figures I have taken the earliest time, whether shown by the D, H or V trace. The S times derived in this way are remarkable in that out of 13 occasions—two S records are omitted—S never once came last, while on nine occasions it was either first or second. It ought to have been last on three occasions, on which the disturbance is regarded as travelling in the easterly direction. It would be interesting to know in this connection the amplitude of the complete movements at S as compared to those at the other stations. C showed the opposite tendency to S. From its position C, according to the theory, should have come second on the list in about half the storms, and never last. In reality, on 11 out of the 15 occasions it was either last or next last. I may add that I was able to identify 10 of the 15 disturbances with reasonable certainty on the Kew curves, and on 8 of these 10 occasions the time which I deduced from the Kew curve—keeping myself purposely ignorant of Mr. Faris' exact times—came next to the C time. The Kew time, as the figure shows, should on the theory, have always come between the P and H times, and never next to the C time. I do not, however, claim an accuracy of ±0·5 minute in my measurements.

Dr. Bauer, so far as I can judge, seems persuaded that Mr. Faris' figures demonstrate the truth of his conclusion, that disturbances normally are propagated round the earth, sometimes eastward, sometimes westward, the time of a complete revolution averaging about 3¾ minutes. Whether this is Mr. Faris' own view I feel some doubts. His statement at the foot of p. 103 of the paper already mentioned certainly suggests a belief that he has proved that magnetic disturbances do not begin simultaneously at different places. At the top, however, of his p. 104 he says "the impression of the simultaneity of magnetic storms . . . may now be stated as not proven." A verdict of "not proven" means in Scotland—the only part of this realm where it is, I believe, a recognised legal term—that a jury has held the evidence insufficient to justify a verdict either of "guilty" or "not guilty." If this is what Mr. Faris means, it is a not unreasonable position to adopt.

As regards the future, I think Dr. Bauer's conclusions could be adequately tested by a careful comparison of curves from selected stations fairly encircling the globe, choosing if possible stations whose time measurements are specially reliable. So far as position is concerned, the following would seem an adequate choice: Apia (172°W.), Honolulu (158°W.), Sitka (135°W.), Toronto (79°W.), or Cheltenham (77°W.), Porto Rico (65°W.), two or three of the best stations in western and central Europe, Pavlovsk (30°E.), Tiflis (45°E.), Mauritius (58°E.), Alibag (73°E.), Toungoo (96°E.), Batavia (107°E.), Lukiapang or Manila (each 121°E.), Melbourne (145°E.) and Christchurch, N.Z. (173°E.).

I should also advise that attention be given to the magnitude and direction of the commencing disturbances as well as to their time of occurrence. If they are due to overhead currents, either in the geographical or the magnetic equator, then in arctic or antarctic stations the horizontal component should tend to be small compared to that in equatorial stations, and the vertical component to become dominant. This was not true of the sudden commencements observed at the station of the National Antarctic Expedition in 1902-3. If sudden commencements are propagated in the way and with the velocity Dr. Bauer supposes, the phenomena in high latitudes must be very curious indeed. An observer, for instance, within a few hundred yards of either pole could easily keep pace with the disturbance.

ABSTRACT.

The question of the simultaneity of magnetic disturbances recorded at different stations has recently been discussed by Dr. Bauer and Mr. Faris. A good many magnetic storms have so-called "sudden commencements." As regards these "sudden" changes three things are conceivable: They may be absolutely simultaneous at different stations; there may be a very small difference of time corresponding to the rate of propagation of electromagnetic waves; or, finally, there may be, as Dr. Bauer concludes, longer intervals, amounting to several minutes, for stations remote from one another. Dr. Bauer concludes that Mr. Faris figures demonstrate the truth of his theory that disturbances normally are propagated round the earth, sometimes eastward, sometimes westward, the time of a complete revolution averaging about 3¾ minutes. The author of the present paper discusses the weaknesses of Dr. Bauer's theory. He points out that the theory could be adequately tested only by a careful comparison of curves from selected stations fairly encircling the globe, choosing, if possible, stations whose time measurements are specially reliable.

V. *On Cusped Waves of Light and the Theory of the Rainbow*

By W. B. Morton, *M.A.*, *Queen's University, Belfast.*

First Received September 9, 1910. Received in Revised Form and Read November 11, 1910.

Introduction.

There is a tendency at the present time to base the teaching of optics on the conception of the wave rather than on that of the ray. In this connection Prof. R. W. Wood[*] has published interesting drawings of the cupsed waves which arise when light is reflected at a spherical mirror. There is another important case in which similar waves occur—viz., the passage of light through a spherical drop of water in the formation of the rainbow. It seemed of some interest to obtain the forms of waves in this case. So far as I know attention has hitherto been directed only to the emergent waves and the imaginary forms obtained by continuing these backwards.[†] In the diagrams 1, 2, 3 which accompany this paper are shown the forms assumed by a wave of light, originally plane, in three successive journeys across the drop, with the corresponding emergent forms. The second and third of the latter are associated with the primary and secondary bows. In connection with the drawings I wish to call attention to some characteristics of these waves, and, in particular, to the advantage of the wave method in giving a simple explanation of the main features of the "supernumerary bows."

Description of the Figures.

The waves are drawn for a succession of intervals of time each equal to an eighth of the time taken to cross the diameter of the drop. The numbers attached to the waves show the number of intervals elapsed since the first incidence. In making the drawings the course adopted was to calculate the successive points of incidence of 10 rays falling on the drop at angles of incidence 0°, 10° . . . 90°, and to mark these off by a protractor on a large scale diagram. The points on the wave fronts were then obtained by measuring off a series of equal optical distances along the rays so obtained. The index of refraction was taken as 4/3.

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[*] Wood, "Phil. Mag.," V. 50, p. 148, 1900; also in "Physical Optics."

[†] *See* in particular Möbius Abh. Sächs. Ges. d. Wiss, Vol. XXX., p. 107, 1907.

The terminal rays which enter and leave the drop tangentially are shown as thin lines. The ends of the waves run along these, while the cusps run along the dotted lines which show the parts of the caustic curves lying in the region of the actual waves. Prof. Wood, in calling attention (*loc. cit.*) to this property of the caustic, remarks that he had been unable to find any previous mention of it, so perhaps an historical note may not be out of place here. The property was enunciated in a paper read by R. Potter * before the British Association at its third meeting, at Cambridge, in 1833. The statement will be found in a paper by Potter in the Cambridge Transactions for 1836, with the title "Mathematical Considerations on the Problem of the Rainbow, showing it to belong to Physical Optics." Further on in the same volume is Airy's classical investigation of the same subject. The fact that the caustic is the locus of cusps on the waves is recognised also in later papers by Jamin † and Macé de Lepinay ‡.

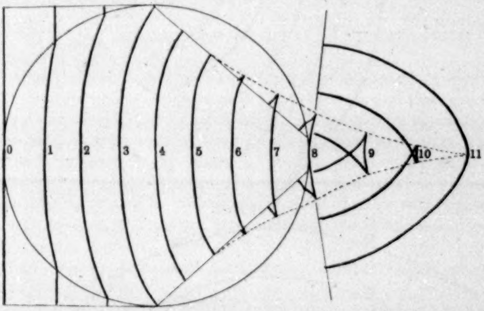

FIG. 1.

Returning to the diagrams one rather curious point may be noticed in connection with the progressive change of form of the surfaces. At certain epochs the original flat circular portion punched out of the incident wave is crumpled up into a com-

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* Potter, Trans. of Camb. Phil. Soc., Vol. VI., p. 141., 1836.
† Jamin, "Comptes Rendus," Vol. LXVI., p. 938, 1868.
‡ Macé de Lepinay, "Journ. de Phys," Vol. VII., p. 209, 1898.

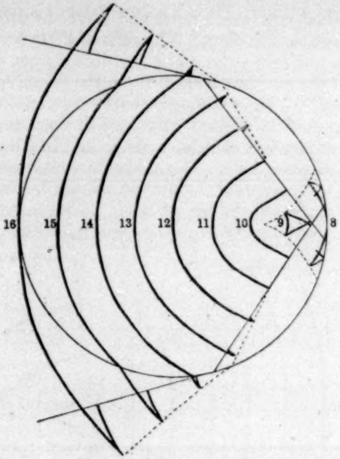
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FIG. 2.

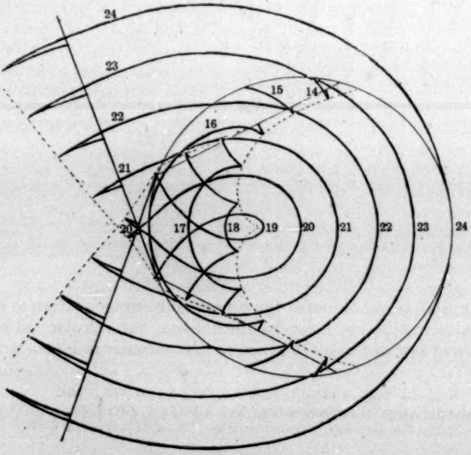
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FIG. 3.

pletely closed surface. This happens when the two terminal rays cross on the axis—*e.g.*, a little before epoch 9 on Fig. 2.

The Emergent Light.

The emergent caustics in Figs. 2 and 3 have as asymptotes the rays of minimum deviation. At a short distance from the drop the caustics practically coincide with the rays in question. The cusps of the waves run along this line and the energy, being condensed near the cusps, is guided along this direction without appreciable diminution, giving the concentration of brightness observed in the bow. For comparison the waves passing out at the back of the drop are shown in Fig. 1 for epochs 9, 10 and 11. It will be seen that the emergent beam of light is now bounded by the terminal rays instead of by the asymptotes of the caustic.

It is at once obvious that the branches of consecutive waves in the neighbourhood of the cusps will intersect, and that interference effects will thus be produced. This explains the occurrence of the maxima and minima of brightness seen in the so-called supernumerary bows. For the purpose of an elementary physical explanation it seems more natural to appeal to the peculiarities of the waves which enter the eye rather than to the form of an imaginary wave surface in the drop, as is done in Mascart's method. We shall see that very simple geometrical considerations show the identity of the results obtained by the two methods.

The idea of the interference of overlapping cusped waves was used by Potter, Jamin and Macé de Lepinay in the papers already referred to. The results obtained by Potter and Jamin were vitiated by the assumption that the phase on the two branches of a wave was the same. Macé de Lepinay investigated analytically the distribution of intensity in the focal plane of a telescope receiving the waves and showed his formulæ to agree with Mascart's.

Phase Relations on the Waves.

The first point to notice in considering the interference of cusped waves is that these do not fulfil the ordinary definition of a wave as a surface over which the disturbance is in the same phase. In passing through a centre of curvature of the wave surface—*i.e.*, through a point of contact of a ray with the caustic the disturbance receives a sudden advance of phase amounting to a quarter period.* When the two centres of curvature coincide, as in the case of a homocentric pencil, the sudden change amounts to a half period, or reversal of phase. What appears to be the corresponding effect in a sound wave may be seen in some of the photographs in which Prof. Wood has obtained exact analogies of optical phenomena. Now, if we consider the two branches of a wave near a cusp it is easy to see that the rays normal to one branch have already passed their contact with the caustic, while those normal to the other branch are still approaching the caustic. Therefore, there is a difference of a quarter period between the phases on the two branches which must be taken into account when investigating the interference due to the crossing of the opposite branches of consecutive waves. For darkness we must have the distance between the branches of a wave equal to $(2n+1)\frac{\lambda}{2}+\frac{\lambda}{4}$.

In this connection it is interesting to notice that the distribution of phases over some of the wave-forms shown in the figures accompanying this paper will be different according as the curves are regarded as sections of cylindrical waves or of surfaces of revolution. In the latter case one of the centres of curvature at any point of a wave lies on the axis ; in the former case one centre of curvature is at infinity. So in the case of a wave which is a surface of revolution an advance of a quarter period occurs when the wave crosses the axis, at a node on the plane section. Instances of this are seen in the internal and external waves numbered 9 in diagrams 1 and 2. If the wave is cylindrical there are only two phases over it, the advancing front being a quarter period behind the rest of the wave. But if the diagram represents a spherical drop there are three different phases, the front part being a quarter period behind the portions between the cusps and the node, and these again the same amount behind the part between the node and the ends. Of course, in the wave the cusps of the plane section are on a ring-shaped edge and the node is a conical point. It is interesting to follow the evolution of these waves into the succeeding forms and to see how the phases become once more equalised as the curve becomes free from singularities.

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* *Cf.* Walker, "Analytical Theory of Light," p. 91, where a reference is given to Poincaré's "Theorie de la lumiére." For an account of recent theory and experiment on this point *see* a paper by W. Möbius, "Ann. d. Phys.," Vol. XXXIII., p. 79, 1910. In the immediate neighbourhood of the focus there are rapid oscillations of phase, so that in the above discussion we exclude a small anomalous region very close to the cusp.

Application to the Case of the Rainbow.

In order to examine the conditions determining the intensity of illumination in a given direction, suppose an eye, adapted for parallel light, to look in the direction A′P or B′Q (Fig. 4). Let AB′ be a plane perpendicular to this direction. ACB and A′C′B′ are two waves such that the A branch of the first and the B branch of the second both touch the plane AB′. Then the disturbances from the elements at A and B′ will reach a point on the retina together. The geometrical distance between the two waves is AA′ or BB′, but taking into account the difference of phase between the two branches of a wave the optical path-difference becomes $BB' - \frac{\lambda}{4}$. Ignoring any difference of intensity between the elements A and B′ we find as a first approximation that there will be maximum intensity if $BB' = n\lambda + \frac{\lambda}{4}$, minimum if $BB' = (2n+1)\frac{\lambda}{2} + \frac{\lambda}{4}$. BB′ is the distance between the parallel tangent planes drawn to the two branches of one wave—*e.g.*, the tangent planes at the points A and B. It evidently corresponds to the spacing of the branches at a large distance from the drop.

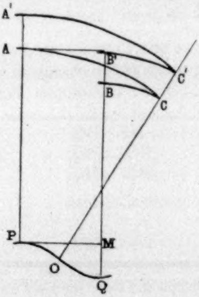

FIG. 4.

Comparison with Mascart's Method.

Mascart, following Airy, supposes the emergent waves continued backwards in a series of parallel surfaces, till a specially simple form, without cusps, is reached. All the waves have points of inflection on the asymptote to the caustic.* This special surface is so placed that the tangent at the inflection on its section passes through the centre of the drop. In the neighbourhood of the inflection the form of the section approximates to that of a cubical parabola. It is represented by POQ on Fig. 4, O being the point of inflection and OCC′ the asymptote to the caustic. The figure is, of course, not drawn to scale. Airy's exact solution was obtained by integrating over this special surface in accordance with Huygens' principle. Mascart showed that an approximation to the correct result could be got by supposing the effect in a direction, such as PA or QB, to be due to the interference of disturbances sent from sources at the points P and Q, the poles of the wave corresponding to this direction. The geometrical path difference is MQ. From this $\frac{\lambda}{4}$ is deducted because the wave is convex at one pole and concave at the other, a statement which is equivalent in effect to recognition of the change of phase in passing a centre of curvature. Now, the curves POQ and ACB are parallel, therefore,

$$AP = BQ,$$
$$B'M = BQ,$$
$$BB' = MQ,$$

so we reach the same result as before.

ABSTRACT.

Diagrams were shown of the forms assumed by a plane wave of light falling on a spherical raindrop and twice reflected from the interior of the drop, as well as the waves emerging from the drop. The waves in general have cuspidal edges which run along the caustic surfaces. This relation between the caustic and the cusps on the waves was pointed out by Wood in connection with the similar waves produced by reflection at a spherical surface. It had been noticed earlier by Potter, Jamin and Macé de Lepinay. The phase over a wave of this type is not constant, the two portions on opposite sides of a cusp differing in general by a quarter period. Attention was drawn to the advantage of regarding the distribution of light in the rainbow as the consequence of the interference of the cusped waves which run down to the observer's eye along the direction of minimum deviation. This way of looking at the matter is shown to be equivalent to Mascart's approximate method of explana-

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* It is the reversal of curvature on crossing the asymptote which makes the two branches of the emergent waves appear concave in the same direction. The portion between asymptote and caustic, where the branches are oppositely curved, is too narrow to show on the diagram.

tion of the formation of the supernumerary bows by interference of disturbances coming from the two poles on the special wave-form used by Airy.

DISCUSSION.

Prof. C. H. Lees pointed out that the boundary of the disturbance as it passed through the drop would be subject to diffraction effects. He asked if this would make any material difference in the results arrived at in the paper.

The Author, in reply, stated that diffraction phenomena would not seriously affect the diagrams he had obtained.

VI. *The Afterglow of Electric Discharge.* *By* The Hon. R. J. Strutt, *F.R.S., Professor of Physics, Imperial College of Science, South Kensington.*

Received and Read November 25, 1910.

Section I.—*Introduction.*

It is quite 50 years since Geissler first observed the afterglow which occurs in vacuum tubes containing particular gaseous mixtures. A considerable number of papers have appeared since then containing discussions of the phenomenon, and contributions to its experimental study. A summary will be found in Kayser's "Handbuch der Spectroscopie," Vol. I., p. 249. In addition to the papers mentioned by him reference should be made to two of special interest, *i.e.*, Newall, Proc. Camb. Phil. Soc., IX., p. 295, and Dewar, Proc. Roy. Inst., 1888.

In the latter paper a most valuable and ingenious method of studying the effect is introduced. A powerful air pump is used to draw a regulated current of gas through the vacuum tube. In this way a continuous removal of the gas from the region of discharge is effected, and the afterglow which it emits in passing through another vessel on its way to the pump can be examined continuously and at leisure.

Dr. Gaede's new mechanical rotatory pump (not the mercury pump) has been found very convenient for repeating this experiment on a moderate scale.

There has been considerable difference of opinion as to whether pure oxygen shows a glow or not. Under the conditions of my experiments the glow, if any, is certainly exceedingly faint. With air, on the other hand, a bright yellow glow is obtained, which is improved by enriching the air with oxygen. This is necessarily an optimum mixture, but I have not determined its exact composition. It has no particular significance, as will appear later. Pure nitrogen gives no glow whatever. The glow from air is not, in my experience, appreciably affected by drying the air, or by saturating it with moisture.

Section II.—*Connection of the Glow with Ozone.*

Several previous experimenters have connected the glow with ozone, though generally without expressing any very definite view as to what part ozone played in its production. The following experiments convinced me of the correctness of their opinion.

(1) The tube carrying the glowing gas was heated to about 300° C. ; the glow was extinguished and did not revive to any considerable extent, as the same gas was carried on to a colder part of the tube.* This, a repetition in modified form of previous experiments by Dewar and J. J. Thomson, agrees well with the hypothesis.

(2) The glow is not able to survive passage through a tube cooled in liquid air. I regard this as due to condensation of ozone.

(3) The glow is destroyed by passage over oxides of copper or manganese, in the cold. Ozone, too, is known to be destroyed by these substances. The glow can, to a great extent, survive passage through glass wool, though brought into far more intimate contact with it than with the above oxides, which were in large pieces.

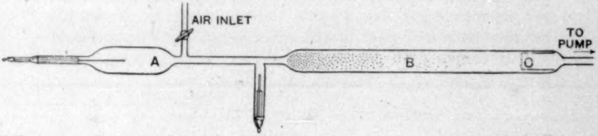

Fig. 1.

(4) Silver gauze is oxidised in the glow to a dead black oxide, and the gauze thus oxidised absolutely destroys the after luminosity of air passing through it. One single layer of the silver gauze is sufficient. A similar partition of iron gauze has little effect.

The apparatus shown in Fig. 1 may be used to exhibit this action of silver. Glowing gas from a vacuum tube, A, passes into the cylindrical vessel B, remaining luminous through most of the length. B contains a partition consisting of one layer of silver gauze, C, carried in a short length of glass tubing, and able easily to slide in the vessel. If the silver partition is moved towards A by tipping the tube, it will be found to cut off the glow completely, although the gas, deprived of glow, of course continues to pass through it. This is a striking experiment. The prejudicial effect of silver on the glow was observed long ago by Sarasin (" Arch. des sc. Phys. et Nat." (2) 34, p. 243), but the conditions of his experiments were, as Kayser remarks, unsatisfactory and the result scarcely convincing.

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* There was, however, a slight recovery which is difficult to account for.

(5) It was found that silver remains bright for any length of time if exposed to the gas current beyond the point at which the glow had died out. This suggests that the cessation of luminosity is conditioned by all the ozone being used up.

(6) The following experiment is confirmatory of this. paper impregnated with thallium hydroxide is rapidly turned brown in the glow, showing that ozone is present in it. Beyond the glow, no ozone can be detected by this test.

(7) Dewar (*loc. cit.*) has observed that hydrocarbons introduced into the discharge destroy the glow. Air mixed with a small proportion of benzole vapour was led into the discharge; although the spectrum still showed little but nitrogen bands, the after glow had disappeared. Silver placed in the issuing gas now remained quite bright. It appears that the action of a hydrocarbon is to prevent the delivery of any ozone. It is probably used up in oxidising the hydrocarbon.

Section III.—*Nature of the Glow.*

The experiments so far recorded show that the glow involves consumption of ozone. It is therefore natural to regard it as a flame of low temperature, arising from the oxidation of some other body by ozone. The next series of experiments were directed to determining the nature of the other body. Ozone was produced in a vacuum tube, through which a current of rarified oxygen passed, in itself producing little or no after-glow. The ozone thus produced was allowed to mix, in a Y-shaped tube, with any other gas which it was desired to test, on its way to the pump.

(1) No effect was observed on mixing the ozone with nitrogen, whether the latter had just passed through a vacuum discharge tube or not.

(2) The second branch of the Y-tube (Fig. 2) was next supplied with a current of air which had just passed through a second vacuum discharge tube, B, quite independent of the first one, A, in which the ozone was produced. This air current was deprived of glow by passing through a silver gauge partition, C, before meeting the ozone current from the other tube. Each of the confluent streams was by itself dark. The

characteristic glow was produced at the point of confluence, and it disappeared if the discharge was stopped in either tube. It may be inferred, then, that air after passage through the discharge contains some constituent which can be oxidised by ozone, producing the glow. This constituent could not well be anything but one of the lower oxides of nitrogen, produced by the discharge.

(3) No effect was produced by mixing nitrous oxide with the stream of ozone, but with nitric oxide (NO) a glow was obtained in the Y-tube of the characteristic yellow colour, and far brighter than anything that had been seen in the previous experiments. It will be understood that no discharge was passed through the nitric oxide, which was merely fed in as a tributary to the stream of oxygen which had been through a discharge.

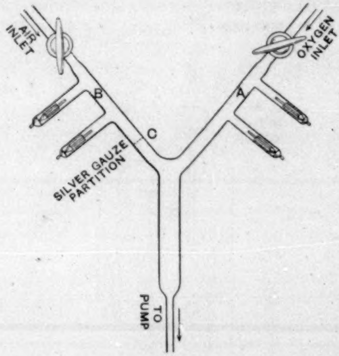

FIG. 2.

The apparatus shown in Fig. 3, serves well to show the experiment to an audience. Ozone is produced at a low pressure in the vacuum tube A. A current of it passes through the jet B into the vessel C, for which a separating funnel, or rather a separating cylinder, of the largest size, answers well. A gentle current of nitric oxide is fed in through D, and enters C by an annular opening concentric with B. The gases are led out to the pump through E. With a small ozone feed, a very bright glow of almost spherical form surrounded by a misty halo may be seen issuing from the jet. If the oxygen feed is increased the glow gradually takes on the form of a pointed flame, similar in shape to that of the oxyhydrogen blowpipe, and showing a well-defined inner cone of small luminosity.

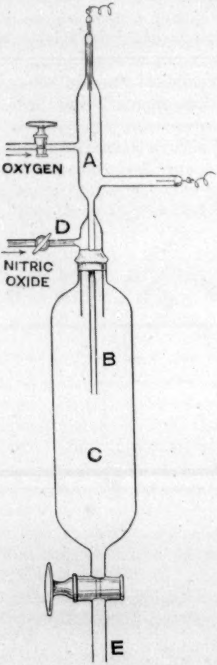

Fig. 3.

I conclude, then, that the afterglow results from these three distinct processes:

(*a*) Formation of ozone in the discharge;

(*b*) Formation of nitric oxide in the discharge;

(*c*) Oxidation of nitric oxide by ozone, after the discharge is over.

Section IV.—*Further Experiments.*

It was proved by the above experiments that ozone straight from a vacuum discharge was capable of producing the glow. An experiment was tried to determine whether the residual ionisation of the oxygen had any connection with the glow. A stream of oxygen from the discharge was passed for three or four minutes through a U-tube (A, Fig. 4), cooled in liquid air, to condense the ozone produced. This U-tube was then shut off by stopcocks B and C, and allowed to warm up. On admitting a small volume of nitric oxide from the space between the taps D and E, a momentary flash of the characteristic yellow colour was observed. Ionisation by the discharge is thus shown not to be directly concerned, for the glow can be obtained long after the discharge is over, and the ions have recombined.

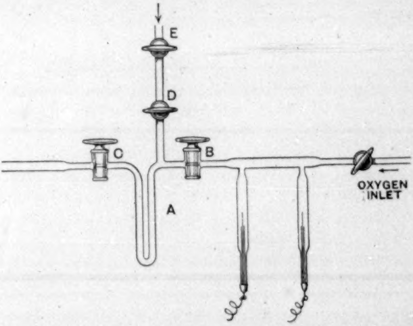

FIG. 4.

I have not been able to obtain the glow by leading oxygen at atmospheric pressure through a Siemens ozone tube, and then into nitric oxide. On reducing the pressure to a few millimetres the glow is obtained, but much less successfully than with the vacuum tube. I am not yet in a position to explain this, but the point will be investigated further.

The temperature of the glow is certainly very low. I have not been able to detect any rise of temperature with certainty, but accurate observations are difficult, since the gas stream is heated several degrees in passing through the low-pressure discharge. The rise of temperature due to the glow itself was found to be certainly less than ½° C., under the particular pressure conditions of the experiment.

The spectrum of the nitric oxide glow has been photographed with a small quartz spectrograph, with the kind assistance of Mr. Eagle. It was found to be continuous, and to extend from λ4,200—λ6,700. It doubtless extends beyond that into the infra red. The continuous character of the spectrum confirms Newall's observations.

Section V.—*Conclusion and Summary.*

It is proposed to study the action of ozone on other easily oxidisable gases. This will probably reveal the origin of various other kinds of afterglow. For instance, the blue one obtained by Newall (*loc. cit.*) in mixtures containing oxygen and sulphur. It is hoped also to study more closely the electrical and other properties of these ozone-supported flames, which have probably a far lower temperature than any others equally bright.

The main conclusion of this paper is that the ordinary yellow afterglow is due to oxidation of nitric oxide by ozone.

NOTE ADDED DECEMBER 20TH.—It should have been mentioned that Sir James Dewar returned to the subject of the afterglow at the Royal Institution in the summer of 1909 with additional illustrations. (*See* "Engineering," June 18th, 1909.) He has, I understand, pursued the subject since, and has arrived independently at some of the present results.

ABSTRACT.

It has long been known that when the electric discharge has passed at low pressure through certain gaseous mixtures a luminosity survives for some seconds after the discharge has been turned off. A greatly improved method of experimenting on the phenomenon was introduced by Dewar (" Proc." Roy. Inst., 1888). A powerful air pump is used to draw a regulated current of gas through the vacuum tube. In this way a continuous removal of the gas from the region of discharge is effected,

and the afterglow which it emits in passing through another vessel on its way to the pump can be examined continuously and at leisure.

There has been considerable difference of opinion as to whether pure oxygen shows a glow or not. Under the conditions of the author's experiments, the glow, if any, is certainly exceedingly faint. With air, on the other hand, a bright yellow glow is obtained, which is improved by enriching the air with oxygen. Pure nitrogen gives no glow whatever.

Several previous experimenters have connected the glow with ozone, though generally without expressing any very definite view as to what part ozone played in its production. The chief evidence for this hitherto has been that the glow is only obtained where oxygen is present, and that it is destroyed by heat. The following additional evidence was obtained.

1. The glow cannot survive passage through a tube cooled in liquid air. This is regarded as due to condensation of ozone.

2. It is destroyed by passage over oxides of copper, manganese and silver. Ozone, too, is known to be destroyed by these substances.

3. While the glowing gas oxidises bright silver, the gas current beyond the point at which the glow has died out does not do so. Disappearance of the glow is simultaneous with disappearance of ozone from the gas.

The glow is thus shown to involve consumption of ozone. It is, therefore, natural to regard it as a flame of low temperature, arising from the oxidation of some other body by ozone. The next experiments were made to determine the nature of this other body. A current of ozone from a vacuum tube fed with oxygen was allowed to mix, in a Y-shaped tube, with any other gas which it was desired to test, on its way to the pump.

Nitrogen or ordinary air added to the ozone gave no effect, but air, which had been through an independent discharge, and had been deprived of its original glow by silver oxide, was found to glow again on mixing with ozone. Clearly then some body is produced in air by the discharge whose oxidation is responsible for the glow.

This body proves to be nitric oxide. On leading a current of this gas into the ozone stream a very brilliant glow was obtained, of the characteristic yellow colour. By suitable arrangements this glow can be produced in the form of a pointed flame, with dark inner cone. The glow is not associated with a sensible rise of temperature. Condensing the ozone with liquid air, allowing it to re-evaporate, and admitting nitric oxide to it, a yellow flash can be obtained, long after the electric discharge is over. The glow is purely chemical in its origin.

Ozone from the Siemens tube used at atmospheric pressure seems incapable of yielding the glow when mixed with nitric oxide. This may be due to the low percentage of ozone present. Further investigation is in progress on this point, and also on the possible production of a glow in other cases of oxidation by ozone.

The main conclusion of the present paper is that the ordinary yellow afterglow is due to oxidation of nitric oxide by ozone.

DISCUSSION.

Dr. J. A. **Harker** asked if the presence or absence of water vapour had any effect upon the results obtained.

The **Author** replied that the same results were obtained with air saturated with water vapour and air dried by means of phosphoric anhydride.

VII. *The Approximate Solution of Various Boundary Problems by Surface Integration* combined with Freehand Graphs.*

By L. F. RICHARDSON.

RECEIVED MARCH 21, 1910. RECEIVED IN REVISED FORM NOVEMBER 1, 1910. READ NOVEMBER 25, 1910.

§ 1. General.

§ 2. $(\partial^4/\partial x^4+2\,\partial^4/\partial x^2\partial y^2+\partial^4/\partial y^4)\gamma=0$ in the body; and on the boundary γ and $\dfrac{\partial\gamma}{\partial n}$ given.

§ 2·1. General.

§ 2·2. Example—Stress in a stretched notched plate.

§1. The method of solving boundary problems now to be described, consists in drawing, at a guess, a rough graph of the lines along which the integral is constant, and testing its accuracy by perimeter integration applied (in the approximate form of summation) to circles (or other simple figures) enclosed within the given boundary, in such a way as to find the integral at the centre of each circle, in terms of the integral and its space rates round the circumference; then embodying the corrections so found in a new graph and repeating the process until improvement becomes slow. In doing this one begins with a circle as large as will go into the region, or if the region is long and narrow, with several such circles. This gives a first approximation to the integral in the centre of the region. The graph is then re-drawn, and circles of half the radius of the first are used to test the parts extending from the centre to the boundary. Then, after re-drawing, still smaller circles are used for testing nooks and corners. Next the largest size of circles is used again, and so on.

The above process is for equations such as $\nabla^2\phi=0$, $\nabla^4\phi=0$, but when we have on the right-hand side of the equations not zero but a known function of ϕ and of the independents, then a certain volume integral has to be taken as well as the perimeter integrals. This can be calculated with sufficient accuracy by adding up a number of arithmetical values. When, as commonly, these are the values of a function of two functions of position, then the work is made easier by drawing a graph of the contours of one of the functions on tracing paper and laying it over a graph of the contours of the other.

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* The particular kind of surface integration referred to is that which gives the value, at a single point inside the surface, of the integral of an equation which holds throughout the volume inside the surface. When, by symmetry, it is only necessary to apply it to a line lying in the surface it will be called "perimeter integration," *not* contour integration as the word "contour" is reserved to mean a line along which a function of position is constant.

The drawing ensures that the function shall satisfy the right boundary conditions, shall have any necessary discontinuities and shall elsewhere be " smooth." The testing by perimeter and volume integration and consequent adjustment ensures that it shall satisfy the particular differential equation obtaining throughout the body. The accuracy of the result may be improved, in the case of certain differential equations, by converting it into a table of numbers and then making successive arithmetical approximations.* Further, whenever it has been decided to attack a problem by this arithmetical method, it will generally be quickest first to improve the preliminary assumption by graphs and perimeter integration.

Errors in graphs of integrals of $\nabla^2\phi=0$ are easily seen because the shape of the chequers into which the contours of ϕ and the orthogonal curves divide the field is a known function of their position and direction †. When, however, we have to deal with rather more complicated equations such as $\nabla^2\phi=$constant, $\nabla^2\phi=k^2\phi$ or $\nabla^4\phi=0$, the shape of the chequers is no longer a known function of their position and direction, but, instead, certain relations have to be satisfied by the differences of the ratio of length to breadth of adjoining chequers, relations which are usually too subtle for the eye to perceive. Instead of attempting to perceive them one may use contour integration in the manner described. The same may also, of course, be applied to $\nabla^2\phi=0$.

An account of Green's functions connected with equation $\nabla^{2m}\phi=0$, where m is an integer, will be found in two Papers by T. Boggio‡. He shows, among other things, that

$$0=\int(u\nabla^{2m}v-v\nabla^{2m}u)d\,(\text{volume})$$
$$-\sum_1^m{}_i\int\left\{\nabla^{2i-2}u\frac{d\nabla^{2m-2i}v}{dn}-\nabla^{2m-2i}v\frac{d\nabla^{2i-2}u}{dn}\right\}d\,(\text{surface}),\quad(1)$$

where dn is an element of the outwardly directed normal.

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* L. F. Richardson, " Phil. Trans." A (1910).

† This statement applies only when ϕ is a function of two co-ordinates, these co-ordinates being of a certain common kind; *see* L. F. Richardson, " Phil. Mag.," February, 1908.

‡ Accademia Reale delle Scienze di Torino, 1900. Rendiconti del Circolo Matematico di Palermo, 1905.

§ 2·1. Let us consider more particularly the equation

$$\nabla_1{}^4\gamma=\{\partial^4/\partial x^4+2\,\partial^4/\partial x^2\partial y^2+\partial^4/\partial y^4\}\gamma=0, \quad . \quad . \quad . \quad (2)$$

which is the one satisfied by the stress function in plain strain, * by the stream function in a viscous liquid moving under certain conditions, and by the displacement in a plane plate bent by forces at its edge.

Putting $m=2$ the formula (1) becomes

$$\int(u\nabla^4 v-v\nabla^4 u)d\text{ (volume)}$$

$$=\int\left(u\frac{d\nabla^2 v}{dn}-\nabla^2 v\frac{du}{dn}+\nabla^2 u\frac{dv}{dn}-v\frac{d\nabla^2 u}{dn}\right)d\text{ (surface).} \qquad (3)$$

Then, if $\nabla^4 v=0$ everywhere except at one point P, where it becomes infinite in such a way that the integral of it over any volume, including this point, is $+1$, then

$$\int u\nabla^4 vd\text{ (volume)}=\text{the value of } u \text{ at P.}$$

If also v and $\frac{\partial v}{\partial n}$ vanish on the boundary then

$$u\text{ at P}=\int\left(u\frac{d\nabla^2 v}{dn}-\frac{du}{dn}\nabla^2 v\right)d\text{ (surface)}+\int v\nabla^4 ud\text{ (volume).} \quad (4)$$

Here u is supposed to be the integral which we require and v a known function which is used to find u.

It is assumed that we are dealing with cases in which $\nabla^4 u$ either vanishes or is a given function of u and of the co-ordinates, so that the volume integral can easily be estimated by adding up values at a number of points.

The special case which we chiefly require is that of a circle with P at the centre. Let the radius be a, then

$$v=\frac{1}{8\pi}\left[r^2\log\frac{r}{a}+\tfrac{1}{2}(a^2-r^2)\right]\dagger. \quad . \quad . \quad . \quad . \quad (5)$$

Forming $\nabla^2 v$ and $\frac{\partial}{\partial r}\nabla^2 v$ at $r=a$ it is found that, if $\nabla^4\gamma=0$, then

γ at centre of circle=(mean value of γ on circumference)

$-$(half radius)× $\left(\text{mean value of } \frac{\partial\gamma}{\partial r} \text{ on circumference}\right)$. (5A)

To determine these mean values on a graph one divides the circumference into a number of parts and adds up the values at their middle points arithmetically.

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* Love, "Theory of Elasticity," edition 1906, p. 201.

† Love, "Elasticity," 1906 edition, § 314.

Another special case which may be required is that where the boundary is an infinite straight line $y=0$ and P is at $x=0$, $y=a$. Then

$$v=\frac{1}{16\pi}[x^2+(y+a)^2][\log\{x^2+(y+a)^2\}-\log\{x^2+(y-a)^2\}]\ ^*$$
$$+\text{an arbitrary linear function}:$$

it follows that γ at $x=0$, $y=a$ is equal to

$$\int_{-\infty}^{+\infty}\left[(\gamma)_{y=0}\frac{2a^3}{\pi(x^2+a^2)^2}-\left(\frac{\partial\gamma}{\partial n}\right)_{y=0}\frac{a^2}{\pi(x^2+a^2)}\right]dx.$$

The forms of v are known for many more general shapes of boundary and with P anywhere. Thus O. Venske (Göttingen Nachrichten, 1891) gives v for the circle, circular ring, space between intersecting straight lines, parallel straight lines and the space bounded by radii and concentric circles. But these will seldom be required for the graphic method.

§ 2·2. *Example.*—A thin elastic plate of the shape shown in Fig. 1 is stretched by forces applied uniformly over its ends. Required to make a graph of the stress function. The plate is 24 units long, 10 wide, and the semicircular notches have a radius of 2·1 and are placed centrally †. If x and y are co-ordinates in the plane of the plate then the stresses are $X_x=\frac{\partial^2\gamma}{\partial y^2}$, $Y_y=\frac{\partial^2\gamma}{\partial x^2}$, $X_y=Y_x=-\frac{\partial^2\gamma}{\partial x\partial y}$, where γ is the stress-function satisfying $(\partial^4/\partial x^4+2\,\partial^4/\partial x^2\partial y^2+\partial^4/\partial y^4)\gamma=0$ ‡. Effect of the thickness of the plate is here neglected. The y axis is taken parallel to the length of the plate. Over the ends $Y_y=$constant, therefore, $\frac{\partial^2\gamma}{\partial x^2}=$constant. Now, arbitrary values may be given to γ, $\frac{\partial\gamma}{\partial x}$ and $\frac{\partial\gamma}{\partial y}$ at any one point without affecting the stresses. So let γ, $\frac{\partial\gamma}{\partial x}$, $\frac{\partial\gamma}{\partial y}$ vanish at the centre of one end. Then, over this end $\gamma=Ax^2$, where A is a constant and x is measured from the centre of the end. Further, X_y vanishes over the end, and, therefore, by integrating $X_y=-\frac{\partial^2\gamma}{\partial x\partial y}$, it follows that $\frac{\partial\gamma}{\partial y}$ vanishes also. Now, it may be shown § that when the boundary stresses and the external forces, such as gravity acting on the internal matter, vanish, then $\frac{\partial\gamma}{\partial x}$ and $\frac{\partial\gamma}{\partial y}$ are constant round the boundary. These conditions are satisfied in the present case except over the ends of the plate. Consequently, if we imagine a length, γ, set up normal to the plane x, y so as to form a surface, then on the longer edges of the strip and round the curves of the neck this surface is the plane which touches the surface $\gamma=Ax^2$ at the extremity of the end. The distribution of γ is obviously symmetrical about the lines through the centre of the plate parallel to the straight sides and ends. Take the origin at the centre. The following is a record of the actual process by which the results to be given at the end were obtained.

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* Love, "Elasticity," 1906 edition, § 314.

† This problem is taken from a note by G. H. Gulliver, "Nature," April 14, 1910, Figs. 5 and 6.

‡ Michell, "Proc." Lond. Math. Soc., xxi. Love, "Theory of Elasticity," ed. 1906.

§ "Phil. Trans.," A Vol., 210, pp. 330—331.

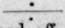
Short pieces of the contours of the surface $\gamma=Ax^2$ were first marked on the ends of the boundary of the drawing, and over the rest of the boundary were marked the ends of the contours of the plane which has $\gamma=A(5)^2$ at the straight parts of the longer edges and which has $\frac{\partial\gamma}{\partial x}=2A.5$; $\frac{\partial\gamma}{\partial y}=0$. In the drawing A was taken to be 2/5. Consequently the equal and opposite forces acting on the ends of the bar, being each equal to

$$\int_{x=-5}^{x=+5}\frac{\partial^2\gamma}{\partial x^2}\,dx, \qquad \text{are} \qquad \Big[4x/5\Big]_{-5}^{+5}=8.$$

This completed the boundary conditions. Next, the ends of the contours were joined across by lines drawn at a guess in Fig. 1A. The value of γ at the centre of the plate was unknown, so that the dotted line $\gamma=1$ was particularly uncertain. The only places at which γ vanishes are the points marked 0. Now, the large circle was drawn and divided into equal segments by dots. The values of γ at the centres of these segments, starting from the left, were read off, thus—

0·9, 5·0, 9·0, 9.0, 4·0, 1·5, average 4·90.

In reading off values of $\frac{\partial\gamma}{\partial r}$ it is a help to have a piece of tracing paper with a line ruled on it, which is placed as tangent to the circle. The values of γ at two dots a half unit on either



side of the tangent are read off and differenced. In other places it is more convenient to measure the distance along a radius between two contours, and to use for $\frac{\partial\gamma}{\partial r}$ the reciprocal of this distance multiplied by the difference in γ between the said contours. Again, at or very near the boundary, $\frac{\partial\gamma}{\partial x}$ and $\frac{\partial\gamma}{\partial y}$ are given, and it is then often most accurate to take the direction cosines of the radius from a book of tables and so calculate $\frac{\partial\gamma}{\partial r}$. By these methods the following values of $\frac{\partial\gamma}{\partial r}$ were quickly found at the centres of the segments, reading from the left

$$0{\cdot}15,\ 2{\cdot}00,\ 3{\cdot}8,\ 3{\cdot}8,\ 2{\cdot}35,\ \text{zero, mean } 2{\cdot}01.$$

Therefore, by equation (5),

$$\gamma \text{ at centre} = 4{\cdot}9 - \frac{5}{2} \times 2{\cdot}01 = -0{\cdot}12,$$

instead of about $+0{\cdot}6$, as indicated directly by the figure. Accordingly a new drawing was made in the quarter of Fig. 1 marked B, and in this γ was made negative in the centre of the plate. It hardly seemed necessary to alter $\gamma = 2, 4, 6, 8, 10$. The mean values of γ and $\frac{\partial\gamma}{\partial r}$ were thereby changed so as to make $\gamma = -0{\cdot}5$ at the centre of the large circle, and this was also the value given directly by the graph as near as one could tell.

Next a circle with centre at the origin and radius 2·9 was taken and divided into segments as before. At the centres of these segments

$$\gamma = -0{\cdot}5,\ +0{\cdot}8,\ +1{\cdot}7,\ \text{mean } +0{\cdot}67,$$

$$\frac{\partial\gamma}{\partial r} = +1{\cdot}9,\ +0{\cdot}95,\ +0{\cdot}15,\ \text{mean } 1{\cdot}00.$$

Therefore, by equation (5),

$$\gamma \text{ at centre of the plate} = +0{\cdot}67 - \frac{2{\cdot}9}{2} \times 1{\cdot}00 = -0{\cdot}78.$$

Next a circle with radius 2·5 and centre at $y = 4$, $x = 2{\cdot}5$ was taken. The value of γ at the centre of this, as given directly by the figure is 1·6. On the circumference, in counterclockwise order from the right, were found

$$\gamma = 2{\cdot}7,\ 7{\cdot}0,\ 9{\cdot}5,\ 9{\cdot}4,\ 7{\cdot}0,\ 3{\cdot}6,\ 1{\cdot}2,\ -0{\cdot}1,\ -0{\cdot}5,\ -0{\cdot}7,\ -0{\cdot}4,\ +0{\cdot}5,\ \text{mean } 3{\cdot}27,$$

$$\frac{\partial\gamma}{\partial r} = 0{\cdot}9,\ 2{\cdot}9,\ 3{\cdot}85,\ 3{\cdot}85,\ 2{\cdot}5,\ 0{\cdot}85,\ -0{\cdot}15,\ -0{\cdot}4,\ -0{\cdot}1,\ -0{\cdot}1,\ -0{\cdot}4 - 0{\cdot}4,\ \text{mean } 1{\cdot}11;$$

whence, by equation (5), γ at the centre $= 3{\cdot}27 - \frac{2{\cdot}5}{2} \times 1{\cdot}11 = 1{\cdot}88$ instead of 1·6, as found directly. The graph was adjusted to

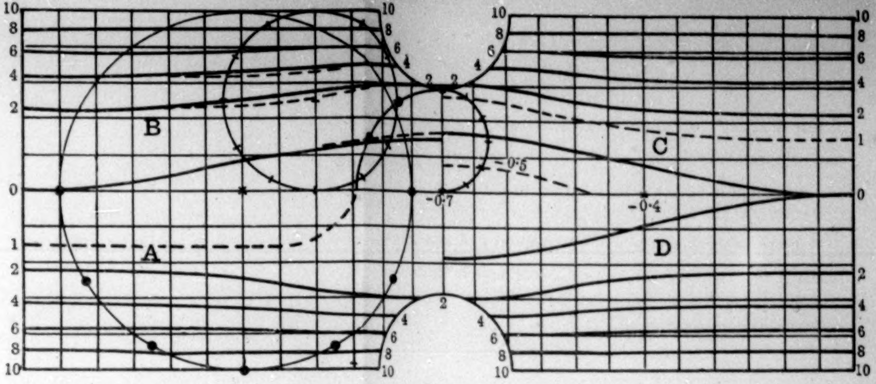

10
8
6
4
2
0
1
B
A
C
D
-0·5
-0·7
-0·4

FIG. 1.

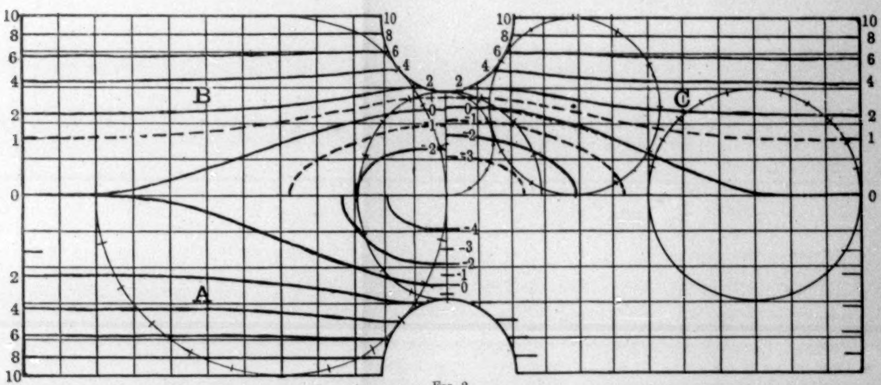

10
8
6
4
2
1
0
B
A
C
0
-1
-2
-3
-4

FIG. 2.

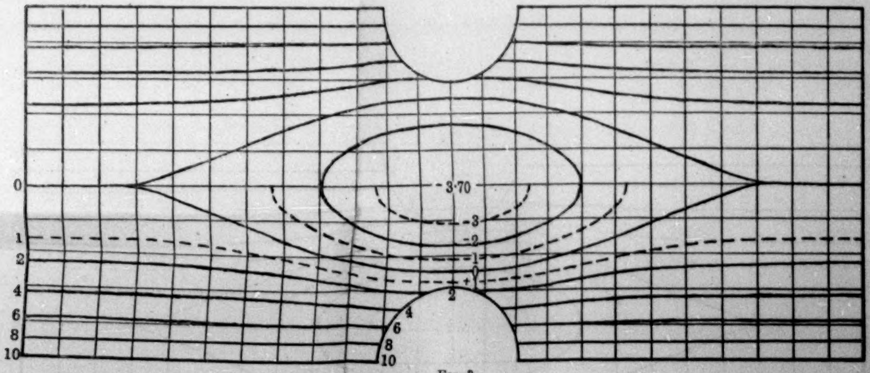

0
1
2
4
6
8
10
-3·70
-3
-2
-1
0
+1

FIG. 3.

To face p. 80]

accord with this value by moving the contour $\gamma=2$ into the position of the dotted line, so that γ was increased by 0·30. Then, to preserve the smoothness of the function γ, the contour $\gamma=4$ had to be given a similar but smaller displacement. These displacements reduced the mean value of $\frac{\partial\gamma}{\partial r}$ on the circumference by only about 0·01. They increased the mean value of γ by about 0·02. The value at the centre, by perimeter integration, was therefore changed by $0{\cdot}02+\frac{r}{2}(0{\cdot}01)=0{\cdot}03$. So it is seen that the slight displacement of the contour passing nearest to the centre of the circle, with accompanying changes to preserve smoothness, made much less change in the value of γ found by perimeter integration than in the value read off directly, the ratio being as 0·03 to 0·30. The motion of the contours was stable. This property, when it exists, allows improvements to be made rapidly. As a result of the last change we have now, at $x=2{\cdot}5$, $y=4$, $\gamma=1{\cdot}90$ directly and $\gamma=1{\cdot}91$ by perimeter integration. Next, if the contour $\gamma=0$ be displaced into the dotted position the effects on the circle which we have just considered will be slight, the change in γ compensating that in $\frac{\partial\gamma}{\partial r}$. This was done. The value at the origin by perimeter integration became —0·72.

The graph now appeared to be fairly satisfactory. Some further tests were proposed to confirm it, and had they been applied they would doubtless have revealed what was now noticed—namely, that the stress Y_y across the neck of the plate is probably fairly uniform. If quite uniform, then γ is there of the second degree in x. But we have the values of γ and $\frac{\partial\gamma}{\partial x}$ at the boundary $x=2{\cdot}9$, and so find at the centre of the plate $\gamma=-4{\cdot}75$ if Y_y is uniform, instead of —0·7 given by perimeter integration. It was at once clear that either Y_y was not uniform or else the figure was seriously out. To decide which, the latest graph was copied into the quarter C of Fig. 1. An auxiliary diagram, Fig. 4, showing γ as a function of x along $y=0$ was used to find the points where $\gamma=1$ and —0·5, and the contours through these points were then marked with dots in Fig 1c.

A circle was taken with centre at $x=1{\cdot}45$, $y=0$ passing through the origin and touching the boundary. At its centre perimeter integration gave —0·32, whereas the value on the graph is there —0·1. Suppose that to correct this discrepancy the zero line were shifted into the new position shown in Fig. 1D. This shift would cause, on the circumference of the circle which we have just considered, an increase in $\frac{\partial\gamma}{\partial r}$ and a decrease in γ, and would, therefore, create a demand for a further displacement in the same direction. The motion of the contours would be unstable.

It was, therefore, necessary to make a large displacement, preferably one overshooting the position of stable equilibrium of the contours. A fresh start was accordingly made from the assumption of uniform stress Y_y across $y=0$. (*See* Fig. 2A.) Here the contours $\gamma=2, 4, 6, 8$ were simply copied from Fig. 1C. The others were guessed. Perimeter integrations were then made around two circles in the same way as in the examples above and with the following results:—

Co-ordinates of centre.		Radius.	Value at centre by perimeter integration.	Value at centre directly from the graph.
x	y			
0	0	2·9	−4·30	−4·75
0	5	5·0	−1·34	−0·8

These two discrepancies can be simultaneously reduced by making γ less negative at the centre of the plate. The amount of change required was not apparent, but for trial a value half way between that in Figs. 1C and 2A was taken—namely, $\frac{1}{2}(-4{\cdot}7-0{\cdot}7)=-2{\cdot}7$. The corresponding graph is Fig. 2B. Taking again the large circle with centre at $x=0$, $y=5$ and $r=5$, it was found on its circumference that the changes from Fig. 2A increase the mean of γ by 0·28 and the mean of $\frac{\partial\gamma}{\partial r}$ by −0·01. Therefore, γ at the centre should be increased from −1·34 to $-1{\cdot}34+0{\cdot}28+2{\cdot}5\times0{\cdot}01=-1{\cdot}04$. Actually it is −0·9. Next from the circle with centre at the origin and radius 2·9 it was found that γ should be −3·13 instead of −2·7 as assumed. Both these tests on Fig. 2B point to the true value at the origin being more negative than −2·7. Accordingly −3·7 was next tried. Fig. 2C is the corresponding graph. In making it the curve marked 1C in Fig. 4 was first drawn to look suitable and was then used to determine the ends of the contours in Fig. 2C along the y axis. The lines $\gamma=4, 6, 8$ are the same as in the preceding graph; $\gamma=2$ has been slightly straightened. Around the circumference of the large circle with centre $x=0, y=5$ and radius 5 the distribution in Fig. 2C is simply the average of those in Figs. 2A and 2B; so γ at its centre was made equal to the average of the values found by perimeter integration from Figs. 2A and 2B—namely, 1·19. Next the following tests were applied :—

Centre of circle.		Radius.	Angle between values used in summations.	γ at centre.		
x	y			By perimeter integration.	Directly.	Diff.
0	0	2·9	22°·5	−3·71	−3·70	−0·01
1·45	0	1·45	30°	−2·46	−2·40	−0·06
2·5	3·85	2·5	30°	+1·35	+1·40	−0·05
0	9·0	3·0	30°	+0·09	0·00	+0·09
			15°	+0·035		+0·035

The differences are all less than 0·1, which is 0·73 per cent. of the total range of γ in the figure.

Enough has now been done to illustrate the process. Fig. 4

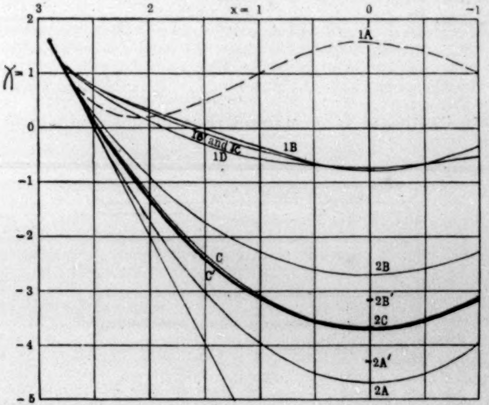


FIG. 4.

shows the successive stages at a glance. The final contours have been copied from Fig. 2C to Fig. 3, where they can be seen more clearly. In copying it was found desirable, in order to improve the smoothness, to shift the line $\gamma=0$ into the position shown dotted in Fig. 2c. The effect of this on the errors tabulated above will be less than 0·1 of γ.

From Fig. 3 the following relations between the stresses in the neck have been deduced. As they depend on second differences they are necessarily very inaccurate, and are to be regarded merely as suggestive. If a greater accuracy were required it would be best either to work with a larger diagram (Figs. 1, 2, 3 are reproductions of drawings 10 cm. × 24 cm.), or else to read from the graph a table of numbers and treat them by successive arithmetical approximations *. To return to the stresses in the neck. In the following table δ^2 is the operator which takes the second difference of the tabulated numbers. Along $y=0$:—

at $x=$	$-1{\cdot}45$	0	$+1{\cdot}45$	$+2{\cdot}90$
$\gamma=$	$-2{\cdot}46$	$-3{\cdot}70$	$-2{\cdot}46$	$+1{\cdot}60$
Here the value $\gamma=-2{\cdot}46$ was found by perimeter integration. *See* table on opposite page.				
herefore $\delta^2\gamma=$	...	$+2{\cdot}48$	$+2{\cdot}82$	...
herefore $\frac{\delta^2\gamma}{\delta x^2}=$	...	$+1{\cdot}18$	$+1{\cdot}34$	...

But the mean value of the stress $\frac{\partial^2\gamma}{\partial x^2}=\mathrm{Y}_y$ is the total force 8. (*see* p. 79) divided by the width of the neck 5·9—that is, 1·355.

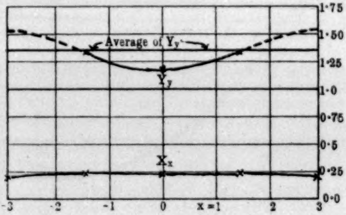


Fig. 5.

To give this mean value Y_y must increase as the edge of the plate is approached. This is indicated in Fig. 5. The shear Y_x

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* L. F. Richardson, "Phil. Trans.", A., 1910, § 3·2.

vanishes by symmetry along $y=0$. The principal stress $X_x=\frac{\partial^2\gamma}{\partial y^2}$, at the origin, must be to $\frac{\partial^2\gamma}{\partial x^2}$ approximately inversely as the squares of the axes of the ellipse $\gamma=-3$ on Fig. 3. By measuring the ellipse its semi-axes were found to be 1·07 and 2·38. Therefore, $X_x=1{\cdot}18\times\left(\frac{1{\cdot}07}{2{\cdot}38}\right)^2=0{\cdot}24$ at the origin.

Again along $y=0$, since $\frac{\partial\gamma}{\partial y}=0$,

$$\frac{\partial^2\gamma}{\partial y^2}=(\text{curvature of line } \gamma=\text{const.})\times\frac{\partial\gamma}{\partial x}.$$

Hence at $x=$	1·45	1·90
$X_x=\frac{\partial^2\gamma}{\partial y^2}=$	0·24	0·20

In conclusion, I have pleasure in thanking Prof. A. E. H. Love for references to Papers about $\nabla^4\gamma=0$ and Dr. C. Chree for reading and criticising the manuscript.

ABSTRACT.

By boundary problems are here meant problems in which an unknown function of position—hereinafter called the integral—has to be found throughout a given region from the knowledge that it satisfies a given differential equation—hereinafter called the body equation—throughout the region, and that the integral and (if necessary) its space-rates are given on the bounding surface in such a way as to make it determinate throughout the region.

The method described applies to a wide range of body equations, and to any shape of boundary which can be represented satisfactorily by an outline drawn on paper. The discussion is for simplicity confined to integrals, which can be represented by a single graph showing a family of lines, along successive members of which the integral has successive constant values.

The method of solution depends on a knowledge of the value of the integral at a point inside some simple figure—say at the centre of a circle—as a function of the integral and (if necessary) its space-rates on the perimeter of the figure.

A drawing is made showing the boundary, and the contours are sketched in so as to satisfy the boundary equations, but elsewhere merely by guesswork. This graph is then tested by circles of various sizes and variously centred, and is adjusted accordingly. The testing and adjustment are repeated until sufficient accuracy is obtained or until improvement becomes too slow to be worth while. Some skill in freehand drawing is required.

As an example a rough determination is made of the stress-function in a notched plate, of special shape, when in tension.

The Electric Stress at which Ionisation Begins in Air. By ALEXANDER RUSSELL, *M.A.*, *D.Sc.*, *M.I.E.E.*

RECEIVED NOVEMBER 10, 1910. READ NOVEMBER 25, 1910.
RECEIVED IN REVISED FORM DECEMBER 20, 1910.

1.—*Introduction.*

Since the author read his Paper* on the electric strength of air in 1905 considerable additions have been made to our knowledge of the sparking voltages, in air at atmospheric pressure, between electrodes of various shapes. These results prove that when suitable precautions are taken this voltage has a perfectly definite value. It is now generally admitted that at atmospheric pressures the maximum electric stress is one of the main factors governing the discharge when the electrodes are at appreciable distances apart. The value of this quantity, however, when computed in the ordinary way by electrostatic theory, is found to vary with the size of the electrodes. It is much greater, for example, with small electrodes than for large electrodes, and, so far as the author is aware, no satisfactory explanation of this fact has been given.

In the preceding Paper it was shown that when equal spherical electrodes of 5 cm. diameter were used the values obtained for R_{max}, the maximum value of the electric stress at the instant of the discharge, are approximately equal to 38 kv. per centimetre for sparking distances varying from 0·3 cm. to 10 cm. The values of $R_{max.}$ for smaller distances are appreciably greater than 38, and for very minute distances $R_{max.}$ attains a very high value. If E be the disruptive voltage, and if we suppose that $E-350$ is the voltage which is effective in producing the discharge, where 350 is the minimum sparking P.D. in air, we considerably increase the range over which $R_{max.}$ is practically constant, and so we extend the practical usefulness of the mathematical formulæ. In what follows, however, we shall make no use of this assumption, but base our calculations, as in the postscript to the preceding Paper, on ordinary electrostatic theory. Before doing this it will be of interest to give an explanation of certain well-known experiments with electrodes at microscopic distances apart which have often been quoted to disprove the existence of a minimum sparking P.D.

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* "Phil. Mag." [6], Vol. II., p. 258, 1906.

2.—*The Minimum Sparking P.D.*

J. E. Almy* has given a direct experimental proof of the existence of a minimum sparking P.D. of about 350 volts. Those experimenters, therefore, who obtained sparking voltages smaller than this when the electrodes were only a micron or so apart must have mistaken the rush of current due to the actual contact of the electrodes for the rush of current which ensues when there is a spark.

Let us suppose that x is the micrometer reading, and that the electrodes approach one another a distance $x-\xi$ owing to the electrostatic attraction when the P.D. between them is established. In the case of equal spherical electrodes, the author† has shown that when the ratio ξ/a of the distance between them to the radius of either is of the order $1/10^4$, the electrostatic attraction can be represented by the expression $\lambda a E^2/8\xi$, with a maximum inaccuracy of less than three parts in 10,000, where λ is the dielectric coefficient of the medium in which they are immersed and E is the P.D. between them. If we assume Hooke's law, the restoring force may be written $\alpha(x-\xi)$, where α is a constant. Hence the value of ξ is given by

$$\lambda\frac{aE^2}{8\xi}-\alpha(x-\xi)=0.$$

The value of ξ, which is greater than $x/2$, corresponds to the stable position of equilibrium. As we increase the voltage, the roots of the quadratic become equal when $E^2=(2\alpha/\lambda a)x^2$. In this case the equilibrium is neutral, and the slightest increase in the value of E will cause the gap to close up, the electrostatic attraction overpowering the restoring force. Hence, if we plot out the values of E and x for which the gap closes up, we get the line whose equation is

$$E=x\sqrt{\frac{2\alpha}{\lambda a}}.$$

For values of E, therefore, less than 350 the graph of E and x will be a straight line, starting from the origin and making an angle $\tan^{-1}(2\alpha/\lambda a)^{1/2}$ with the axis of X. When, however, E equals 350 volts, a spark takes place between points on the electrodes which are at a certain distance, β, apart, which is, in general, greater than the minimum distance between the electrodes at this instant. Until the distance between the electrodes exceeds β, a spark takes place when the voltage is 350. Hence this part of the graph is a straight line parallel to the axis of X. For greater distances of the electrodes the value of E has to be increased in order to obtain a spark.

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* "Phil. Mag." [6], Vol. XVI., p. 456, 1908.

† "Proc." Roy. Soc., A., Vol. LXXXII., p. 524, 1909.

An examination of the experimental results* obtained for "sparking voltages" when the electrodes are at microscopic distances apart will show that they are in accordance with this theory.

3.—*Concentric Cylindrical Electrodes.*

In a valuable Paper on the electric strength of air, by J. B. Whitehead,† experiments on the voltage at which ionisation takes place at the surface of a wire placed along the axis of a metallic tube closed at the ends are described. Near each end of this cylindrical tube holes are drilled. Air enters by these holes and leaves through holes grouped together not far from the upper end. The circulation of the air is maintained by an exhaust fan. The air as it escapes passes over a suitable discharge terminal connected with a gold-leaf electroscope. The wire and tube are connected with the terminals of a transformer, so that a high alternating P.D. can be established between them.

In the experiments a low P.D. was first applied and then gradually increased. At a certain sharply defined voltage the leaves of the electroscope suddenly collapse. This is obviously due to the ionisation which accompanies the electrical breakdown of the air surrounding and close to the wire. Similarly, when the P.D. is gradually diminished from above the critical value, the ionisation suddenly becomes negligibly small when the voltage falls below this value.

Experiments are also described which show that the critical voltage is practically unaffected by the rate at which the air flows through the tube. The rate at which the ions reach the electroscope terminal is affected by this velocity, but for speeds varying from 0 to 370 ft. per minute the rate of discharge suddenly increases enormously at practically the same voltage. In the experiments referred to below, a velocity of about 150 ft. per minute was employed.

It was found that the critical voltage varied with the temperature. At 21°C. there was a fall of 0·22 per cent. in the critical voltage for a rise in temperature of one degree. This is in good agreement with Ryan's value of 0·27 per cent.* obtained by experiments on the voltage at which the corona first appears.

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* *See* P. E. Shaw, "Proc." Phys. Soc., Vol. XX., p. 289 and the references given there.

† "Proc." Amer. Inst. Elec. Eng., Vol. XXIX., p. 1,059, July, 1910.

The values of the maximum critical surface intensity $R_{max.}$ were computed by the well-known formula,

$$R_{max.} = E/\{a \log (b/a)\},$$

where E is the maximum P.D. between the wire and the tube, a the radius of the wire and b the inner radius of the tube. The values of $R_{max.}$ computed in this way from the experimental results were found to be independent of the metals of which the wire and tube were made. Varying the value of b did not alter the value of $R_{max.}$, as we might have anticipated by the ordinary electrostatic theory. But the values of $R_{max.}$ varied largely with the radius a of the wire. For instance, when the wire is 0·089 cm. in diameter, $R_{max.}$ is 77·1 kv. per centimetre, but when the diameter is 0·475 cm., $R_{max.}$ is only 51·4 kv./cm. It is important, therefore, to investigate the law which connects $R_{max.}$ with a.

Temperature 21°*C.* *Pressure* 76 *cm.*

Diam. of wire in cm.	Metal.	Diam. of tube in cm.	Metal of tube.	$R_{max.}$ (observed) kv./cm.	$R_{max.}$ computed by (1).
0·089	Cu	4·9	Brass	77·10	76·91
0·122	,,	4·9	,,	70·95	70·36
0·122	,,	4·9	,,	70·80	
0·156	,,	4·9	,,	65·88	65·93
0·156	,,	4·9	,,	65·88	
0·205	,,	4·9	,,	61·35	61·59
0·205	,,	4·9	,,	61·50	
0·205	,,	6·35	,,	62·08	61·59
0·205	,,	6·35	,,	61·78	
0·205	,,	6·35	,,	61·68	
0·254	Al.	6·35	,,	58·75	58·59
0·276	Cu.	6·35	,,	58·08	57·51
0·276	,,	6·35	,,	58·00	
0·276	,,	6·35	,,	57·74	
0·276	,,	6·35	,,	58·16	
0·276	,,	6·35	,,	58·08	
0·276	,,	9·52	Steel	57·65	57·51
0·325	Al.	6·35	Brass	55·00	55·51
0·347	Cu.	6·35	,,	54·50	54·75
0·3405	,,	9·52	Steel	55·10	54·97
0·399	Steel	9·52	,,	53·05	53·21
0·475	,,	9·52	,,	51·40	51·44

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* Russell, "Phil. Mag.," October, 1910.

4.—*Formula for the Maximum Electric Stress.*

The author has recently investigated a formula for the cooling of a wire maintained at a constant temperature in a draught of air.* The heat emissivity per square centimetre of the surface of the wire in this case is of the form $A+B/\sqrt{a}$, where A is the heat radiated directly and $B/\sqrt{a}$ is the heat carried away by the convection currents. This suggested trying whether a similar expression would give the value of $R_{\text{max.}}$ in Whitehead's experiments. It was found that the formula

$$R_{\text{max.}}=32+13{\cdot}4/\sqrt{a} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where R_{max} is in kilovolts per centimetre, gives all Whitehead's results within the limits of experimental error.

We conclude from the preceding table that the formula gives the values of $R_{\text{max.}}$ for all wires having diameters lying between 0·05 cm. and 0·5 cm., with a maximum inaccuracy of less than 1 per cent.

When the corona appears, substances affected by cathode rays, oxide of zinc for instance, become phosphorescent when placed near it. We may suppose, therefore, that electromagnetic waves capable of producing ionisation, as well as light waves, are being produced by the actions taking place in the corona. In the author's opinion this is due to collisions between positive and negative ions in the thin layer of the corona. The energy being radiated into space is the electrostatic energy of the ions which is set free when they collide and form a neutral system.

Let us suppose that n is the concentration of the positive and negative ions in the corona. By the molecular theory of gases we know that the number of collisions per second between pairs of ions will equal αn^2, where α is a constant. The total number of collisions per second per unit length of the wire will be $2\pi a(\alpha n^2)$, and we may assume that violent ionisation begins when this attains a certain constant value. Hence, so far as the size of the inner wire is concerned, the critical value of n varies inversely as $\sqrt{a}$. If we suppose that the value of the electric stress R at the surface of the corona is constant, and remember that the concentration n of the ions in the corona is proportional to the potential gradient across it, we get

$$R_{\text{max.}}=R+\frac{B}{\sqrt{a}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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* "Trans." Amer. Inst. Elec. Eng., Vol. XXIII., 1904.

where R and B are constants. We have seen above that the experimental results can be exactly expressed by a formula of this nature.

5.—*Parallel Cylindrical Electrodes.*

In some of Steinmetz's experiments* two equal and slightly bent cylindrical rods were used as electrodes. To a first approximation, we may consider the electrostatic field between them as the same as that due to two parallel rods. It has to be carefully noticed that this case is very different from the case of a cylindrical rod and a coaxial tube. In the latter case we have a uniform corona all round the inner cylinder, but in the former the corona appears on the sides of the cylinders which face one another. The electric stress on the air at the surface of the cylinders is a maximum along the lines where the plane containing their axes cuts the inner sides of their surfaces. When they are close together this stress varies very rapidly from point to point along the circumference of a perpendicular section. It can readily be shown from the usual formulæ† that at any point, P, on the circumference of either cylinder we have

$$R=\frac{2q}{a}\cdot\frac{\sqrt{x^2+4ax}}{x+4a\sin^2(\theta/2)}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where q is the charge per unit length on the surfaces of the cylinders, x the distance between them, a the radius of either and θ the angle which the perpendicular from P on the axis of the cylinder makes with the plane which contains the axes of the cylinders. The value of q is given by

$$q=\frac{E}{4\cosh^{-1}\{(x+2a)/2a\}}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If R′ be the electric stress at the highest or lowest points of the cylinders, we have

$$\frac{R'}{R_{\text{max.}}}=\frac{x}{x+2a}; \quad \frac{R_{\text{min.}}}{R'}=\frac{x+2a}{x+4a}. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It will be seen that, when x is small, R′ is very small compared with $R_{\text{max.}}$ Hence it is only when x is a large multiple of a that the conditions approximate to those in Whitehead's experiments.

In one experiment Steinmetz used two cylindrical rods each

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* "Trans." Amer. Inst. Elec. Eng., Vol. XV., p. 281, 1898.

† Russell, "Alternating Currents," Vol. I., p. 102.

0·795 cm. in diameter, and the amplitude factor of the alternating P.D. was 1·42. Hence, computing $R_{max.}$ in the usual way,* we obtain the following table :—

Two Cylindrical Electrodes.
$2a=0{\cdot}795$; $R_{max.}=(1{\cdot}42V/x)f$.

x in cm.	f.	V in effective kilovolts.	$R_{max.}$ in kv./cm.
0·165	1·07	4·78	44·0
0·20	1·08	5·70	43·1
0·43	1·18	10·85	42·0
0·88	1·35	18·55	40·4
1·115	1·43	23·7	43·2
1·19	1·46	22·9	39·8
1·435	1·55	23·0	35·3
1·75	1·66	30·5	41·0
2·13	1·80	34·3	41·1
2·36	1·88	33·1	37·4
3·00	2·07	41·5	40·7
3·66	2·28	47·3	41·8
4·37	2·53	51·4	42·3
5·38	2·84	53·8	40·3
6·65	3·17	65·0	44·0
7·16	3·30	67·8	44·4
8·03	3·61	71·0	45·3
9·53	3·96	73·3	43·3
10·5	4·21	77·4	44·1
15·0	5·45	93·6	48·3

The mean of the values of $R_{max.}$ given above is 42·1. It will be seen that, although in order to get $R_{max.}$ we multiply the average potential gradient at the instant the voltage is a maximum by numbers varying from 1·07 to 5·45, yet the maximum departure from the mean is only about 14 per cent. If the values of V had been smoothed out by "curving" them in the usual way, the variation from 42·1 would only have been a few per cent. This proves that a knowledge of the mean value of $R_{max.}$ is of practical importance.

An examination of the above results shows that R_{max} diminishes from the value 44 when x is 0·165 cm. to a minimum value of about 39 when x is 2·5, it then gradually increases. When x is 15 cm. $R_{min.}/R_{max.}=0{\cdot}904$, and consequently the conditions approximate to those in Whitehead's experiments where the electric stress is uniform over the whole surface of the wire. By formula (1) we find that $R_{max.}=47{\cdot}0$, when $2a=0{\cdot}795$. This is in good agreement with the value 48·3 found above.

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* "Proc." Phys. Soc., Vol. XX., p. 73.

In a second experiment Steinmetz used 2·819 cm. rods.

Two Cylindrical Electrodes.
$2a=2{\cdot}819$; $R_{max.}=(1{\cdot}42V/x)/f$.

x in cm.	f.	V in effective kilovolts.	$R_{max.}$ in kv./cm.
0·24	1·03	5·9	36·2
0·28	1·03	7·0	37·1
0·44	1·05	9·94	33·9
0·48	1·06	11·6	36·6
0·81	1·09	19·3	36·9
1·03	1·11	21·6	32·9
1·25	1·14	27·0	35·0
2·26	1·25	41·0	32·2
3·33	1·37	52·0	30·4
3.78	1·41	60·5	32·0
4·52	1·48	66·2	30·8
4·55	1·49	68·4	31·8
5·23	1·57	74·0	31·5
5·56	1·61	79·0	32·4
6·02	1·65	84·6	32·9
6·45	1·69	93·0	34·6
7·37	1·77	98·5	33·6
8·10	1·82	103·7	33·5
9·45	1·97	118·4	35·1

The mean of the values of R_{max} in the preceding table is 33·7. In this case, also, there are indications that $R_{max.}$ is great when the cylinders are close together, and that it passes through a minimum value as they are separated. This minimum value occurs when x is about 4 cm. Undue importance, however, must not be attached to the above results, as the author has made no attempt in his calculations for f to allow for the slight curvature of the axes of the cylindrical electrodes.

In the last experiment recorded in the above table $R_{min.}/R_{max.}$ is only 0·59. We would, therefore, expect that the value of $R_{max.}$ found by (1) would be different from 35·1. The value found by this formula is 43·3.

8.—*Spherical Electrodes. Kowalski and Rappel's Results.*

A very careful series of experiments on the sparking voltages between equal spherical electrodes has been recently published by Kowalski and Rappel.* It will be interesting, therefore, to take their results and examine the law according to which $R_{max.}$ varies with x and a.

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* "Bull." Acad. Sci. Cracovie, Vol. V., p. 707, May, 1909. Translated in the "Phil. Mag.", Vol. XVIII., p. 699, November, 1909.

If we take their values for $R_{max.}$ and x for any set of experiments with given electrodes we find that they lie very approximately on a smooth curve. The curves for $R_{max.}$ for spherical electrodes are roughly similar to the corresponding curves for cylindrical electrodes. When the electrodes are close together $R_{max.}$ is great; as they are separated it attains a minimum value, and then seems to increase according to a linear law.

9.—*Formula Connecting Electric Stress and Sparking Distance.*

Trying a formula analogous to that for the cooling of a hot sphere in a liquid, the author finds that when $x/a=0{\cdot}1$ the formula

$$R_{max.}=22+24/\sqrt{a} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

agrees sufficiently well with Kowalski's results. When, however, $x/a=0{\cdot}2$, the formula becomes

$$R_{max.}=23+17{\cdot}5/\sqrt{a} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and similar formulæ are obtained for other values of x/a.

The following empirical formula will be found sufficiently accurate for all values of x/a between 0·1 and 2, and for all values of a between 1 cm. and 15 cm., these being the limits for Kowalski's experiments :—

$$R_{max.}=21+10\frac{x}{a}+\frac{11+1{\cdot}3(x/a)}{\sqrt{a}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

It follows from this formula that $R_{max.}$ has a minimum value when

$$x=0{\cdot}36a^{\frac{3}{4}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

For example, when $a=1$,

$$R_{max.}=32+10x+1{\cdot}3/x \quad . \quad . \quad . \quad . \quad . \quad (10)$$

x (cm.).	$R_{max.}$ by (10).	$R_{max.}$ (Kowalski).
0·1	46·0	46·0
0·5	39·3	38·8
1·0	43·3	43·2
1·5	47·9	47·7

Similarly, when a is 5, we have

$$R_{max}=25{\cdot}9+2x+2{\cdot}9/x \quad . \quad . \quad . \quad . \quad (11)$$

x (cm.).	$R_{max.}$ by (11).	$R_{max.}$(Kowalski).
0·5	32·7	32·8
1·0	30·8	30·9
1·5	30·8	30·8
1·8	31·2	31·2

10.—*Formula Connecting Voltage and Sparking Distance.*

For spheres,[*] when x/a is not greater than 4, we have

$$R_{\max.} = \frac{E}{x} f,$$

where $$f = 1 + \frac{x}{3a} + \frac{x^2}{45a^2}\text{, approx.}$$

This formula for f when the spheres are at potentials $+E/2$ and $-E/2$ can be proved by the Euler-Maclaurin sum formula. It is identical with the corresponding formula proved by Schuster[*][†] for the case when the spheres are at potentials E and 0, but its range is much more extended, as the error in our case is less than 1 per cent. even when x is $4a$. Hence, with the same limitations as in formula (8), we have

$$E = \frac{x\,R_{\max.}}{1 + x/3a + x^2/45a^2}. \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

11.—*Comparison of the Maximum Electric Stress with the Electrostatic Attraction at the Instant of the Discharge.*

It is of interest to compare the maximum electric stress at the instant of the discharge with the electrostatic attraction between the electrodes at the same instant. It would be suggestive, for instance, if the minimum value of $R_{\max.}$ occurred when the electrostatic attraction between the electrodes was a maximum. Making the assumption that $R_{\max.}$ is constant for electrodes of given size, the author finds that the electrostatic attraction between them is a maximum when $x = 1{\cdot}2a$ approximately. The first of the following tables indicates that when x has this value the electrostatic attraction is a maximum.

Kowalski's Experiments with 2 cm. Spheres.

x (cm.).	$R_{\max.}$ (Kowalski).	Attraction in dynes.	x (cm.).	$R_{\max.}$ (Kowalski).	Attraction in dynes.
0·085	47·6	237	0·672	39·8	625
0·136	42·9	297	0·875	41·9	690
0·177	40·9	325	0·953	42·6	705
0·220	39·9	367	1·07	44·0	726
0·316	38·5	435	1·35	46·4	721
0·372	38·5	478	1·53	47·9	708
0·494	38·7	543	1·75	49·0	650
0·537	38·8	563	2·22	51·3	594

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[*] "Phil. Mag." [6], Vol. II., p. 258, 1906.

[†] "Phil. Mag." February, 1890.

The above table shows that $R_{max.}$ has a minimum value when x equals 0·34, and indicates that the attraction has its maximum value, not when x has this value, but when x is 1·2 approximately.

Kowalski's Experiments with 3·85 *cm. Spheres.*

x (cm.).	$R_{max.}$ (Kowalski).	Attraction in dynes.	x (cm.).	$R_{max.}$ (Kowalski).	Attraction. in dynes.
0·103	44·2	507	0·843	34·9	1,570
0·145	40·9	584	0·929	35·1	1,650
0·181	39·2	653	1·04	35·4	1,740
0·238	37·7	765	1·25	36·0	1,910
0·311	36·3	887	1·37	36·8	1,980
0·387	35·7	891	1·47	37·4	2,050
0·507	34·9	1,180	1·66	38·2	2,140
0·690	34·6	1,400	...	...	...

In this case $R_{max.}$ has its least value when x equals 0·7 cm. approximately; but when x is 1.66, *i.e.*, when x/a is 0·86, the electrostatic attraction is still increasing.

The Author is at present engaged in examining by means of simplified formulæ sparking voltages, obtained with unequal spherical electrodes. He therefore postpones making any general deduction from the above results.

ABSTRACT.

In the "Proceedings" of the American Institution of Electrical Engineers for July, 1910, Prof. J. B. Whitehead publishes the values of the electric stress at which ionization begins in air. The electrodes he used in his experiments consisted of a metal tube and a cylindrical wire coaxial with it. Alternating pressures were employed, and the inner wires used had diameters varying in size from 0·089 to 0·475 cm. If a be the radius of the inner wire, the Author has found that the expression $32 + 13{\cdot}4/\sqrt{a}$ gives all Whitehead's experimental results for the maximum electric stress in kilovolts per centimetre with a maximum inaccuracy of less than 1 per cent. The experiments show that the electric stress at which ionization occurs is independent of the metals used for the electrodes and of the inner radius of the outer tube. It depends merely on the radius of the inner wire. It is shown that Steinmetz's experimental results on the sparking distances between parallel rods are in substantial agreement with Whitehead's figures. Possible reasons for the discrepancies are given. An empirical formula is given which is based on experimental results recently published by Kowalski and Rappel for the sparking voltages between equal spherical electrodes. The results indicate that the electric stress at the moment of discharge has a minimum value when the distance between the electrodes is a certain function of their radius. The author has neglected the currents of electrified air which stream round the electrodes before the discharge takes place. These currents probably modify appreciably

the values obtained for the disruptive stress at the moment of discharge. The striking similarity between the formula for the heat emitted per unit area at the surface of a hot wire cooling in air and the formula for the electric stress at the surface of an electrified wire at which ionization first begins is pointed out.

DISCUSSION.

Dr. C. **Chree** expressed his interest in the Paper, and remarked that when the electrodes were close together electrostatic attraction might produce an actual transfer of matter from one electrode to the other. The effect of the electrostatic attraction on the diminution of the distance between the electrodes would depend upon the nature of the apparatus used.

Prof. **Strutt** remarked that the effects which the Author had discussed in his Paper might be attributed to causes similar to those suggested by Dr. Chree.

Dr. **Erskine-Murray** gave some particulars of Dr. Shaw's experiments on the subject, and asked the Author if he would extend his researches to electrodeless discharges.

IX. *A Demonstration of the Phase Difference between the Primary and Secondary Currents of a Transformer by Means of Simple Apparatus.* *By* F. T. Trouton, *Sc.D., F.R.S., Professor of Physics, University College, London.*

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A simple arrangement, such as described below, serves well for demonstrating Maxwell's relation for the difference in phase between the primary and secondary currents in a coreless transformer, as well as several other interesting points, both with a core and without one.

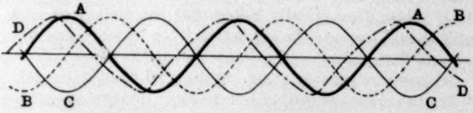
The essential part consists of a little inductive motor of primitive type. Two small horse-shoe electromagnets of slightly different length are placed standing vertically and across each other in such a way that their respective lines of poles are at right angles and in addition are in a horizontal plane. Over these, on a pivot, is placed a copper disc; this can be made to rotate like Arago's disc by the rotary field which is obtained by the passage through the magnets of the primary and secondary currents from a coreless transformer.

The difference in phase of the primary and secondary currents of such a transformer is given by the relation $\rho = \pi/2 + \tan^{-1} Lp/R$ when L and R are the inductance and resistance of the secondary circuit. By introducing additional inductances their ratio can be made large, so that their difference in phase approximates to 180 deg. and consequently we then find that the torque producing rotation of the disc becomes very small or practically vanishes. On the other hand, if the resistance is made relatively large, rotation is obtained; for then the phases of the two component fields being nearly at right angles are in the best condition to produce a rotating field.

If an iron core, consisting of fine wires, is introduced into this transformer the rate of rotation of the disc is generally much reduced; this is brought about by the effective or equivalent phase difference approaching nearer 180 deg., and is due to the hysteresis of the iron. Perhaps a graph giving the wave-forms makes this matter clearer. As only the phases are of interest for the purpose in hand no attempt is made in the diagram to give amplitudes to scale.

If we suppose, for simplicity, that an E.M.F. of simple harmonic type is applied to the primary, we then can take the impressed magnetic flux in the iron core to be also of simple harmonic type. Let this be represented by the full line. The E.M.F. in the secondary is shown by the dotted line and is $\pi/2$ in respect to phase behind the flux. The secondary current may lag behind this by any angle between 0 deg. and 90 deg., depending on the value of $\tan^{-1}(Lp/R)$. We see then that the lag of the secondary behind the flux may vary between 90 deg. and 180 deg. The major limit of the lag is exhibited by the thin line.

In the case of a coreless transformer these are also the limits for the lag of the secondary current behind the primary current, because then the impressed flux and current are identical as to phase; but if there is a core with hysteresis the primary current has a wave form somewhat as shown in the diagram by the unsymmetrical line, and, though no longer a simple harmonic curve, is seen on the whole to be in advance of the impressed flux. Thus we see that the difference in effective phase of the primary and secondary currents should be increased by the introduction of an iron core.



A = Impressed Flux.
C = Secondary Current.
B = Secondary E.M.F.
D = Primary Current with Hysteresis.

Again, it is obvious from the diagram that we should be able to get a difference in phase greater than 180 deg. This is easily realised in the apparatus described. All that is necessary to show it is to arrange, by increasing the induction in the secondary circuit, that the phase difference of the currents without a core is nearly 180 deg., when, on introducing a core, reversal of the rotation is at once obtained.

The apparatus, when in this state, is very suitable for demonstrating the rule that increase of inductance increases the lag while increase of resistance diminishes it. For reversal in direction is, of course, more easily adjudged than simply change in speed.

That the advance of the primary current, or what is the same thing, the lag of the flux carrying with it the secondary current depends upon the amount of hysteresis of the core can be demonstrated by substituting a steel core for the soft iron one. This lag is shown through increased speed of the disc in the reverse direction. By bringing the disc to rest by suitably changing the resistance in the secondary a measure can be obtained of the relative hysteresis of the materials employed for the core of the transformer. Unfortunately, it is necessary to have the material finely sub-divided as the Foucault currents in a solid core complicate the phenomenon, thus limiting its use as a hysteresis tester.

The part played by Foucault currents in the question of phase is best examined experimentally by exaggerating them. This is done by removing the core and substituting in its place a coil through which our exaggerated Foucault currents are to run, but now under control, for we can add resistance or inductance as desired, and there are besides no hysteresis effects to complicate matters.

It is shown theoretically below that the closing of such a circuit should increase the lag between the primary and secondary, and that under certain circumstances the phase difference can be actually greater than 180 deg., so that it should be possible to realise reversal of the direction of rotation without an iron core. Experiment has amply confirmed this.

When, in addition to the primary, there are two secondary windings on a coreless transformer, we have the following relations holding :—

$$L\frac{dC}{dt}+M_1\frac{dC_1}{dt}+M_2\frac{dC_2}{dt}+RC=E\cos pt,$$

$$L_1\frac{dC_1}{dt}+M_1\frac{dC}{dt}+m\frac{dC_2}{dt}+R_1C_1=0,$$

$$L_2\frac{dC_2}{dt}+M_2\frac{dC}{dt}+m\frac{dC_1}{dt}+R_2C_2=0,$$

when L, L_1, L_2, R, R_1, R_2, C, C_1, and C_2 are the inductances, resistances and currents in the three circuits while M_1 and M_2 are the mutual inductances of primary with each secondary and m is that between the secondaries themselves.

To determine the phase difference between the primary current C and, say, C_1, one of the two secondary currents, we can write, adopting exponential notation :—

$$(L_1ip+R_1)C_1+mipC_2=-M_1ipC,$$
$$mipC_1+(L_2ip+R_2)C_2=-M_2ipC.$$

Eliminating C_2, we have

$$\{(R_2L_1+R_1L_2)ip+R_1R_2-(L_1L_2-m^2)p^2\}C_1=\{-M_1R_2ip+(M_1L_2-M_2m)p^2\}C.$$

This gives us for the phase difference between C and C_1

$$\rho=\tan^{-1}\frac{M_1R_2}{(M_1L_2-M_2m)p}+\tan^{-1}\frac{(R_2L_1+R_1L_2)p}{R_1R_2-(L_1L_2-m^2)p^2}.$$

It may be noticed in passing that the value of ρ falls back into Maxwell's expression for the lag of the secondary behind the primary current, namely, $\pi/2+\tan^{-1}(L_1p/R_1)$ when R_2, the resistance of the subsidiary winding is made to grow indefinitely great.

If there is no magnetic leakage M_1L_2 would equal M_2m, provided that none of the inductance L_2 is other than that from the coil wound on the transformer. In which case the first term can never be far from $\pi/2$.

The value of the second term can be made equal to, greater or less than $\pi/2$ by varying the value of L_1, the inductance of the secondary circuit. In the experiment described above, L_1 necessarily includes a considerable inductance external to the transformer, namely, that of the rotatory apparatus, so that $L_1\times L_2$ is always greater than m^2, to which it would be equal were the external inductance absent. For the angle to be greater than $\pi/2$ it is only necessary to make $(L_1L_2-m^2)p^2$ greater than $R_1\times R_2$. In that case the total lag of the secondary current behind the primary current is greater than 180 deg., and we get reversal in the direction of the rotation.

The effect of varying other quantities in the denominator of the second term can be easily examined experimentally. Thus, increasing R_2 is found to produce direct rotation while increase the frequency (p^2) tends to produce reversed rotation, both of which, of course, are in agreement with the expression for the lag.

The hysteresis effect can be greatly modified by subjecting the iron of the core to permanent magnetic fields, which can be done by passing a continuous current through an additional coil wound on our transformer. Moderate fields increase the hysteresis effect so that the lag of the secondary current behind the primary is increased. This can be best observed by arranging first that the lag is almost 180 deg. when, on applying the continuous magnetic field reversal of rotation occurs. Large fields, on the other hand, give rotation in the direct sense.

The first is due to bringing the magnetic cycle arising from the alternating currents into the region represented by the knee of the hysteresis curve; while the second effect is due to the core being permanently saturated, when it acts in respect to the relatively feeble alternating fields as a non-magnetic substance.

DISCUSSION.

Dr. W. E. **Sumpner** remarked that the apparatus showed the effects of phase in a simple and striking way, and was likely to be useful for lecture purposes. But it was not altogether an advantage that so many phenomena can be shown by it. The phase difference between the two currents is due to the complex action of many causes. There is no simple connection between the phase of the primary current and that of the magnetism of the core of the transformer. Alterations in the secondary or tertiary circuit not only affect the relationship between the core magnetism and the secondary circuit, but also between the primary current and the magnetism of the core. This is true whether the transformer core is of air or of iron, but in the latter case, with enclosed iron cores the effects are most complex and do not lend themselves to simple explanations.

Prof. **Silvanus P. Thompson** shared Dr. Sumpner's doubt on certain points. The design of the apparatus took his mind back to one of the early meetings of the Physical Society in 1878 when Mr. Walter Baily showed an identical piece of apparatus for producing Arago's rotations by two electromagnets beneath a copper disc. They were supplied with an imitation of a two-phase alternating current, made by passing currents from a battery through a double revolving commutator. It was in fact the very first two-phase motor.

Prof. G. W. O. **Howe** pointed out that the magnetising winding, by means of which Prof. Trouton magnetises the iron core of the transformer, was really another secondary winding, and if the magnetising current was obtained from a few cells, this winding would be practically short-circuited. The effects produced might therefore be due to the presence of this winding, and not to the fact that the iron core was magnetised.

He considered it preferable to demonstrate the phase relations of the primary and secondary currents of a transformer by means of a double projection oscillograph, with one strip in the primary and the other in the secondary circuit. In the absence of an oscillograph, the present ingenious and easily made device would undoubtedly be of great value.

Mr. A. **Campbell** drew attention to an instrument recently described in the "Philosophical Magazine" which consisted of two wires carrying a small mirror, and crossing at right angles in a magnetic field at right angles to them both. The primary current of a transformer was sent through one wire and the secondary through the other when the spot of light described loops which showed phase difference and hysteresis effects.

The **Author**, in reply to Prof. Howe, said that the magnetising coil employed had a high resistance in order to avoid the effect he mentioned.

X. *A Note on the Experimental Measurement of the High-Frequency Resistance of Wires. By* J. A. FLEMING, *M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London.*

READ JANUARY 27, 1910.

IN a Paper read to the Institution of Electrical Engineers in December, 1909, the author described an apparatus for the measurement of the high-frequency resistance of wires with which many additional experiments have been made lately in the Pender Electrical Laboratory, introducing one or two little improvements which have conduced to greater accuracy.* The apparatus as originally described consisted of a pair of glass tubes T_1T_2 having bends at the bottom full of mercury and closed by air-tight stoppers at the top and bottom (see Fig. 1). These tubes are connected by a lateral inverted syphon made of barometer tube having in it some paraffin oil with an air bubble (*b*) in the centre. The arrangement therefore forms a differential air thermometer and the air bubble serves to show whether the air pressure in both the large tubes is the same. The vertical tubes are about 30 in. in length and 1·5 in. in diameter. In each tube is placed one of two exactly equal wires (W) to be tested. The wire is supported from a copper rod passing air tight through a rubber stopper and the bottom end attached to another copper rod hangs in the mercury in the lower bend by means of which an electric current can be passed through the wires.

Means are provided by a condenser, inductance and discharger for generating electric oscillations of any desired frequency. Also means for passing a continuous current of any strength through either wire. By means of throw-over switches (P_1 P') the high frequency or the continuous current can be passed through either wire in the tubes and measured by means of suitable high-frequency hot-wire ammeters, A_1 A', as described in the Author's previous Paper (*loc. cit.*). Neglecting for the moment some necessary corrections, the experiment made consists in passing a continuous current through one wire and a high-frequency current through the other similar wire and adjusting them until the rate of production of heat in both cases is the same, when a steady condition is reached, as shown by the indicating bubble remaining stationary. If R is the steady resistance of the wire and R′ is its high frequency resistance, and if A and A′ are the equi-heating values of the continuous and high-frequency currents respectively, then for thermal equilibrium we have $A^2R = A'^2R'$, and therefor $R'/R = A^2/A'^2$.

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* *See* J. A. Fleming. "Some Quantitative Measurements in Connection with Radio-telegraphy." "Proceedings of the Institution of Electrical Engineers," London. Vol. XLIV., p. 349, 1910.

One essential condition of success is that the high-frequency current must be measured by an ammeter capable of giving the true root-mean-square value. Such an ammeter has been described by the author in detail in the Paper referred to as read before the Institution of Electrical Engineers.*

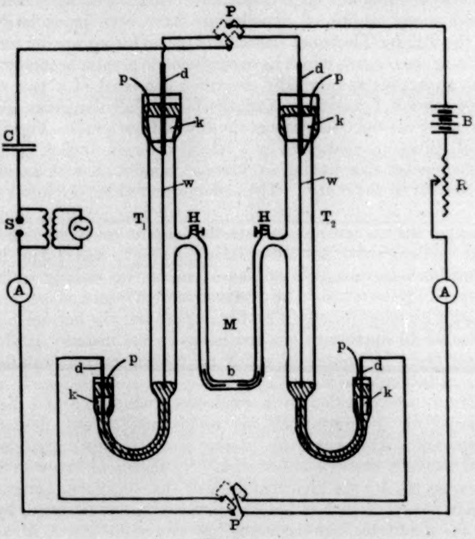

Fig. 1.

There are, however, a number of precautions to be taken in using the apparatus. In the first place, although the wires tested and the tubes in which they are placed are as nearly alike as possible, the conditions of loss of heat are not absolutely the same. There may be a small difference in the emissivity of the wires due to difference in polish, also a difference in superficial area. Again the glass tubes may not absorb or radiate heat quite equally; and therefore the rate of loss of heat from the wires is not quite the same.

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* This hot-wire thermo-electric high-frequency ammeter is now being manufactured by Mr. Robert Paul, of Newton-avenue Works, New Southgate, London, N.

Supposing then that currents are passed through the two wires such that the bubble remains at rest it shows that the air-pressure in the two tubes is equal, and therefore that the air in the two tubes has the same mean temperature. If the experiment is continued until a steady state is reached the rate at which heat is being removed from each wire is proportional to some constant depending on the nature of its surface and some function of the mean temperature of air in the tube. Hence, if we pass a continuous current, A_1, through one wire and a high-frequency current, A', through the other, and if t_1 is this mean temperature in both tubes when the bubble is at rest we shall have

$$\begin{aligned} A_1^2R &= e_1 f(t_1), \qquad \dots\dots\dots \quad (1) \\ A'^2R' &= e_2 f(t_1), \end{aligned}$$

where e_1 and e_2 are constants appertaining to each wire and $f(t_1)$ is some function of the mean temperature of the surrounding air.

Again, if we cause the high-frequency and the continuous current to change wires and vary the value of the continuous current until we again reach a steady state, at which the mean temperature of the air in the tubes is t_2, and if A_2 is the continuous current required to thermally equilibrate the same high-frequency current A', we have

$$\begin{aligned} A_2^2R &= e_2 f(t_2), \qquad \dots\dots\dots \quad (2) \\ A'^2R' &= e_1 f(t_1). \end{aligned}$$

From (1) we have

$$\frac{R'}{R} = \frac{e_2}{e_1}\left(\frac{A_1}{A'}\right)^2 \qquad \dots\dots\dots \quad (3)$$

and from (2) we have

$$\frac{R'}{R} = \frac{e_1}{e_2}\left(\frac{A_2}{A'}\right)^2. \qquad \dots\dots\dots \quad (4)$$

Hence from (3) and (4) we have

$$\frac{R'}{R} = \left(\frac{\sqrt{A_1A_2}}{A'}\right)^2. \qquad \dots\dots\dots \quad (5)$$

In other words, if we equilibrate the same high-frequency currents by continuous currents A_1 and A_2, changing wires in the two measurements, then the ratio of the high frequency to the steady resistance is equal to the ratio of the product of the two direct currents to the mean square value of the high-frequency current. In the former experiments we assumed that the arithmetic mean of the two continuous currents should be used, but it is clearly more correct to take the geometric mean. Against the former experiments on this subject the criticism was made that the calculated value of the high-frequency resistance with which the observed value should be compared should be the high-frequency resistance to damped oscillations which, as Dr. E. H. Barton has shown, is somewhat larger than the high-frequency resistance to persistent oscillations or undamped high-frequency currents.* This correction is a small one, though not absolutely negligible. Hence, in the above formula R' must be understood to mean the resistance to such oscillations as are employed in the experiment. Also, it was asserted that the heating effect due to the high-frequency current was partly due to the charging current of the condenser used.

This last is, however, a quite negligible source of error. The condenser used was a battery of Leyden jars, which, when charged and discharged, gave trains of oscillations, each having about 30 to 50 oscillations or so, as proved by the decrement in that circuit. Hence the actual charging current of the condenser is not more than 3 per cent. or so of the oscillatory current at the most, and its mean-square value is not more than about 0·1 per cent. Moreover, the resistance on which the low-frequency charging current operates is the ordinary or steady resistance of the wire, whilst the resistance to the oscillatory current is the much greater high-frequency resistance. Hence, any correction under this heading is negligible in comparison with other sources of error.

The greatest practical difficulty is to secure a sufficiently steady high-frequency current. This was generated by employing a motor-driven alternator to give current to a large high-tension transformer raising the potential of an alternating current having a frequency of 50 to a potential of 10,000 or 20,000 volts. This voltage was used to charge one or more Leyden jars, which were discharged across a spark-gap, an air-blast on the gap being used to steady the discharge. The frequency of the oscillations so created was measured in each case by a cymometer, and the mean square value of the current by one of the Author's hot-wire thermo-electric ammeters.

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* *See* Dr. E. H. Barton "On the Equivalent Resistance and Inductance of a Wire to an Oscillatory Discharge." "Proc." Phys. Soc., London, 1899, Vol. XVI., p. 400, or "Phil. Mag.," 1899, Vol. XLVII., p. 433.

The voltage of the alternator could be regulated by a resistance in its field so as to alter the number of spark discharges per second. The wires inserted in the tubes for test had in all cases potential wires attached to their ends brought out separately through the stoppers. The purpose of these was to enable a measurement of the resistivity of the wires to be made *in situ* and at the temperature actually used. The experiments were conducted as follows: The two wires of identical materials were selected and cut the right length, and, having been cleaned, the diameters and lengths were carefully measured. They were then attached by solder and screw clips to the holders and inserted in the tubes. After the apparatus was rendered air-tight, continuous currents were passed through the wires and the current and volt drop down these wires measured with the potentiometer, the currents employed being of about the same value as those actually used in the thermal balance. These measurements gave the resistivity (ρ) of the wire. Thus, if A is the continuous current through the wire in amperes and v the volt drop down the wire of length l and section s, the resistivity is given by $\rho = sv/l\mathrm{A}$. Generally speaking, a number of such measurements were made and a curve set out so as to be able to determine the proper value of the resistivity corresponding to any steady current used.

This having been done, the high-frequency current A' was passed through one wire, say, in the left-hand tube, and a continuous current, A_1, passed through the right-hand wire and adjusted until the bubble in the inverted syphon remained at zero, after sufficient time had elapsed to allow the thermal state to become steady. The currents were then reversed, and the alternating passed through the right-hand wire and a continuous current, A_2, through the left-hand wire. The ratio of the product of A_1 and A_2 to A'^2 was then calculated, and this gives the ratio of the high frequency (R') to the steady resistance (R) of the wire.

The wires tested in these experiments were circular sectioned straight wires of copper (No. 30, No. 16, No. 14 S.W.G.), tin, eureka and steel, and two spirals of No. 16 bare copper wire.

For each straight wire the value of the quantity $\sqrt{h}=\pi d\sqrt{\frac{n}{\rho}}$ was calculated, where d is the diameter in centimetres, ρ the resistivity in C.G.S. units corresponding to the mean value of the continuous current A used, and the frequency n of the oscillatory current A′. It has been shown * that if $\sqrt{h}$ has a numerical value in the above units exceeding, say, 10, then for such wires the ratio R′/R is given by a formula due to Dr. A. Russell,† which is nearly equivalent to

$$\frac{R'}{R}=\frac{\sqrt{h}}{2}+\tfrac{1}{4}+\frac{3}{32\sqrt{h}}-\frac{1}{16h\sqrt{h}}.$$

For values of $\sqrt{h}$ in the neighbourhood of 10 and upwards the two first terms suffice.

On the other hand, if $\sqrt{h}$ is less than 10 or so, then the ratio R′/R must be obtained by the aid of a formula called the Ber and Bei formula, first given by Lord Kelvin and reobtained and discussed by Dr. A. Russell.

In these calculations we have used Lord Kelvin's formula, because it has been reduced to numerical form for certain values of $\sqrt{h}$ or its equivalent by Dr. Magnus Maclean.‡ The values of R′/R for the non-magnetic wires for the values of the frequency and current employed have been accordingly calculated in Table, on opposite page.

The correction to be applied for the higher resistance of the wires to damped rather than undamped resistance has not been made in the calculated value of R′/R because the decrement of the oscillations was not observed in every case, but this correction can at most increase the calculated value by about 1 per cent. or less. Dr. Barton has shown that the correction to be applied to Lord Rayleigh's formula for the ratio of R′/R, which is the same as the first term of Dr. Russell's formula is multiplication by a factor $s\sqrt{s+k}$, where

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* *See* J. A. Fleming, "The Principles of Electric Wave Telegraphy and Telephony," 2nd edition, p. 112, 1910, for references to the literature of this subject.

† *See* Dr. A. Russell, "Proc." Phys. Soc., London, Vol. XXI., 1909; also "Phil. Mag.," Vol. XVII., p. 524, 1909, "On the Effective Resistance and Inductance of a Concentric Main."

‡ *See* Lord Kelvin, Presidential Address, "Journal," Inst. Elec. Eng., 1889, Vol. XVIII., p. 35, and Appendix. Dr. Russell has shown that this Table requires recalculating, and some revised Tables have been published by Mr. H. G. Savidge, *see* "Proc." Phys. Soc., London, Vol. XXII., p. 105, 1910.

TABLE I.—*The Measurement of the High-Frequency Resistance of Certain Wires.*

Conductor tested.	Value of $\sqrt{h} = \pi d\sqrt{\frac{n}{\rho}}$	Frequency n.	R.M.S. value of the H.F. current in amps. $=A'$.	Value of the direct current in amperes which balances A' thermally.			Ratio of currents $\frac{A}{A'}$	Ratio of resistances $R'/R=(A/A')^2$.		Remarks.
				Right hand tube $=A_1$.	Left hand tube $=A_2$.	Value of $A=\sqrt{A_1A_2}$.		Measured.	Calculated.	
Bare straight copper wire, No. 30 S.W.G. dia. = 0·03149 cm.	2·415	1,080,000	1·75	1·79	2·48	2·11	1·21	1·45	1·45	Calculated from Lord Kelvin's formula $\rho=1{,}800$ (measured).
Bare straight copper wire, No. 16 S.W.G. dia. = 0·1626 cm.	12·77	1,080,000	5·80	13·1	17·0	14·92	2·57	6·62	6·64	Calculated from Dr. A. Russell's formula $\rho=1{,}730$ (measured).
Bare straight copper wire, No. 14 S.W.G. dia. = 0·198 cm.	15·4	1,060,000	6·70	18·5	19·7	19·1	2·86	8·10	7·95	Calculated from Dr. A. Russell's formula $\rho=1{,}760$ (measured).
Bare straight tin wire pure tin, dia. = 0·0265 inch	2·79	1,060,000	1·85	1·93	2·04	1·98	1·07	1·15	1·13	Calculated from Lord Kelvin's formula $\rho=17{,}000$ (measured).
Bare straight Eureka wire, No. 26 S.W.G. dia. = 0·019 inch	0·623	1,060,000	1·045	1·00	1·08	1·04	0·995	0·99	1·00	Calculated from Lord Kelvin's formula $\rho=23{,}500$ (measured).
Steel wire, 1·0 % C. ... dia. = 0·017 inch	$0{\cdot}906\sqrt{\mu}$	1,060,000	0·530	1·36	1·37	1·363	2·575	6·63	—	$\mu=196$ (calculated). $\rho=23{,}500$ (measured).
Steel wire, 0·8 % C. ... dia. = 0·0156 inch	$0{\cdot}906\sqrt{\mu}$	1,060,000	0·530	1·35	1·38	1·363	2·575	6·63	—	$\mu=196$ (calculated). $\rho=19{,}800$ (measured).
Spiral of No. 16 bare copper wire, 135 turns turns per cm. = 2·65	—	545,000	3·90	10·8	11·05	10·91	2·800	$R''/R=7{\cdot}82$	$R''/R'=1{\cdot}63$	value of R'/R calculated for $n=545{,}000$ by Russell formula $=4{\cdot}80$.
Spiral of No. 16 bare copper wire, 135 turns turns per cm. = 2·65	—	750,000	3·90	9·8	10·15	10·0	2·517	$R''/R=6{\cdot}54$	$R''/R'=1{\cdot}2$	value of R'/R calculated for $n=750{,}000$ by Russell formula $=5{\cdot}54$.

$s=\sqrt{1+k^2}$, and $k=\delta/\pi$, where δ is the semi-period decrement of the oscillations. Experiments made on similar circuits to those used in the case of these measurements showed that δ might be taken to have some value of the order of 0·04, and therefore $k=\delta/\pi$ of the order of $\frac{1}{80}$. Hence, for such a case, $^{s}\sqrt{s+k}=1\cdot007$, or the correction is under 1 per cent. Since the value of R′/R obtained by experiment cannot be repeated with differences much less than 1 per cent., it has not been considered necessary to apply the correction to the calculated values of R′/R, but it can be seen that, as far as the present experiments go, it would, if applied, make the agreement with the observed ratio in some cases rather better and in others rather worse than it is without it. In any case, the conclusion to be drawn is that this experimental method furnishes a value for the ratio which is in very fair agreement with that predetermined by theory in those cases in which the latter can give us a value.

This being so, we can have confidence in it in other instances, such as those of the magnetic metals in which an unknown quantity, viz., the permeability, enters into the value of $\sqrt{h}$. In this case $\sqrt{h}=\pi d\sqrt{\frac{\mu n}{\rho}}$.

Accordingly, for straight wires of the magnetic metals, we have approximately the equation

$$\frac{\mathrm{R}'}{\mathrm{R}}=\tfrac{1}{2}\pi d\sqrt{\frac{\mu n}{\rho}}+\tfrac{1}{4},$$

and this equation can be used to find the value of μ when n, d and ρ are known and R′/R is experimentally determined. Observations were accordingly made with two steel wires of different carbon content about 1 per cent. and 0·8 per cent., using the same methods. It was found that the experimental value of R′/R, corresponding to a frequency of $1\cdot06\times10^6$, came out in both cases the same, viz., 6·63. The diameters of these wires were respectively 0·017 in. and 0·0156 in., and the experimentally determined resistivities 23,500 and 19,800 C.G.S. units, hence $\pi d\sqrt{\frac{n}{\rho}}=0\cdot906$ in each case, and we have the value of the permeability μ from the equation

$$6\cdot63=\tfrac{1}{2}\times0\cdot906\sqrt{\mu}+\tfrac{1}{4}$$

or

$$\mu=196 \text{ for both wires.}$$

Since the radius of the cross-section of each wire is nearly 0·02 cm., and since the root-mean-square value of the current is about 1·3 amperes or 0·13 absolute units, it is clear that the root-mean-square value of the magnetic force H at the surface of the wire is about 13 C.G.S. units. The value of the permeability above found, viz., 196, is a kind of mean value corresponding to an intermittent oscillating magnetic force, and not to any steady value of the magnetic force.

If a magnetic force of 13 C.G.S. units was steadily applied the permeability corresponding to it would be about 900 or 1,000 for such a steel as is here used. Hence we cannot infer from the static magnetisation curve the permeability corresponding to a high frequency intermittent magnetising force any given R.M.S. value, since the actual permeability for such a magnetising force so applied appears to be always much less than for the equal steady force.

In the last place two experiments were made on the high-frequency resistance of spirals of copper wire. It is well known that a close spiral of wire of good conducting material has a larger high frequency resistance than the same wire stretched out straight. Hence, for such a spiral we have three resistances to consider, viz., its steady resistance R, its high frequency resistance R′ and its spiral resistance R″. The ratio of R″/R can be determined in the above-described apparatus as easily as that of a straight wire.

A spiral of copper wire, No. 16 S.W.G., was made having 135 turns and 2·65 turns per centimetre of length. This was tested at two frequencies, viz., $n=545{,}000$ and $n=750{,}000$. In the former case R″/R was found to be 7·82 and in the latter 6·54. From the known value of R′/R for this wire at these frequencies it was found that R″/R′ was for the lower frequency equal to 1·63 and for the higher 1·2. These values show that the ratio R″/R′ appears to diminish with increase of frequency.

The subject of the high-frequency resistance of spirals has already engaged the attention of several mathematicians and experimentalists. The formulæ given so far for predicting it for high frequency currents do not seem to be in good agreement with experimental results.

The latest investigation is that of Dr. J. W. Nicholson (*see* "Proc." Phys. Soc., London, Vol. XXII., p. 114, 1910, "On the Effective Resistance and Inductance of a Helical Coil"). He considers mathematically such a case as is here dealt with experimentally, namely, a circular-sectioned wire wound into a spiral of fairly close turns in one layer and traversed by a high frequency current.

If a is the radius of the cylinder on which the spiral is wound and r is the radius of the section of the wire, and n is the number of turns in a length, z, of the spiral, then the angle α of the spiral is defined by $\tan \alpha = z/2\pi na = \beta/\pi \mathrm{D}$, where β is the distance from turn to turn and D is the diameter of the spiral. When a is greater than $12r$, and the frequency greater than $4{,}900/r^2$, Dr. Nicholson arrives at an expression for the effective resistance of the spiral per unit length of the wire which he gives in the form (*see* his equation (46), p. 128, *loc. cit.*)

$$\mathrm{R} = \left(\frac{\mu p\sigma}{2\pi r^2}\right)^{\frac{1}{2}} \left\{ 1 + \left(\frac{\sigma}{8\pi\mu pr^2}\right)^{\frac{1}{2}} + \frac{3}{4}\frac{\sigma}{8\pi\mu pr^2} \right\}$$

$$- \frac{\sigma}{32\pi a^2} \cos^2\alpha(1-5\sin^2\alpha) \left\{ 1 + \left(\frac{\sigma}{8\pi\mu pr^2}\right)^{\frac{1}{2}} - \frac{9}{4}\left(\frac{\sigma}{8\pi\mu pr^2}\right)^{\frac{3}{2}} \right\}$$

where σ is the resistivity of the wire, μ the permeability, r the radius of section, and $p = 2\pi n$, where n is the frequency. This expression can be simplified as follows: In place of his R put R″ since he uses R to denote the effective resistance per unit of length of the spiral wire, and I have here employed R to signify the steady resistance per unit of length. In place, therefore, of $\sigma/\pi r^2$, write R, since this is the steady or ohmic resistance per unit of length of the wire. Also take $\mu = 1$, as we are dealing with non-magnetic wires, and in place of $\sigma/8\pi pr^2$ write $1/4h$, which makes $\sqrt{h} = 2\pi r\sqrt{\frac{n}{\sigma}} = \pi d\sqrt{\frac{n}{\rho}}$, as above; also write d for $2r$ and D for $2a$.

With these changes, Dr. Nicholson's formula becomes

$$\frac{\mathrm{R}''}{\mathrm{R}} = \left\{ \frac{1}{2}\sqrt{h} + \frac{1}{4} + \frac{3}{32}\frac{1}{\sqrt{h}} \right\} - \frac{1}{32}\frac{d^2}{\mathrm{D}^2}\cos^2\alpha(1-5\sin^2\alpha)$$

$$\cdot \left\{ 1 + \frac{1}{2\sqrt{h}} - \frac{9}{32}\frac{1}{h\sqrt{h}} \right\}.$$

It will be seen that the first three terms in large brackets are the approximate expression, according to Dr. A. Russell's formula, for the ratio of the resistance per unit length of the wire for high-frequency currents to its steady resistance per unit of length, the wire being straight. This ratio is what we have called R′/R. Hence Nicholson's formula becomes

$$\frac{R''}{R}=\frac{R'}{R}-\frac{1}{32}\left(\frac{d}{D}\right)^2\cos^2 a(1-5\sin^2 a)\left\{1+\frac{1}{2\sqrt{h}}-\frac{9}{32\,h}\frac{1}{\sqrt{h}}\right\}$$

when d is the diameter of the wire and D of the spiral. If the angle a is small the term $\cos^2 a(1-5\sin^2 a)$ is nearly unity, and positive in sign, and if h is large the three last terms in large brackets are also not far from unity and positive.

Hence the formula clearly shows that for a value of a less than 26° 35′ R″/R should be *less* than R′/R by a small quantity. Now, this is contradicted by experiment. R″/R is always *greater* than R′/R, and the ratio of R″/R is as shown above, a quantity which is 1·63 or 1·2 for the spiral in question for the two frequencies 545,000 and 750,000.

Also the formula shows that when $a=\sin' 1/\sqrt{5}=26° 35'$ R″/R=R′/R; and that when a is greater than 26° 35′ R″/R is greater than R′/R. The formula is, therefore, qualitatively wrong in certain cases and not in agreement with the results of experiment. The only case in which it agrees with observation as regards ratio of R″ to R′ is when the angle of the spiral is greater than 26° 35′, but this case has little or no practical importance, as the inductance coils of importance are mostly spirals of small angle.

Similar experimental measurements of the ratio R″/R′ made for copper spirals of the same kind by T. P. Black* in 1906 gave results of the same order as those obtained by me, viz., values between 1·2 to 1·89 for frequencies of 10^6 or that order and spirals made of wire 0·15 and 0·3 cm. in diameter.

It can be proved by elementary reasoning, as given by the author in the Paper read to the Institution of Electrical Engineers that the spiralisation of a wire must increase its high-frequency resistance, and this is experimentally confirmed by the measurement here given and by all those of Black. There is no doubt that a fairly close spiral of many turns made of No. 16 copper wire has a resistance to high-frequency currents with frequency $\frac{1}{2}10^6$ to 10^6, which is from 1·5 to 2 times that of the same wire stretched out straight to the same currents. Hence any formula which makes it less or only greater by a very small percentage must be wrong.

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* *See* T. P. Black, "Widerstand von Spulen für Schnelle Elekt Swingungen," "Annalen der Physik," Vol. XIX., p. 157, 1906.

A formula given by Sommerfield gives, as Black has shown, too large a value for R″/R, and another formula given by Cohen has only been tested with rather low-frequency currents.

There seems, therefore, still room for mathematical investigation to obtain for us a formula for the resistance of spiral wires to high-frequency currents, which shall be as much in accord with the results of observations, as are the formulæ given by Lord Kelvin and Dr. Russell for straight conductors. The experimental work for this Paper has been very carefully carried out by Mr. A. D. Peacock, superintended by Mr. G. B. Dyke and by me; and to them I am indebted for the experimental results here given.

ABSTRACT.

The Author refers to a Paper read by him in December, 1909, before the Institution of Electrical Engineers on Quantitative Measurements in Connection with Radio-Telegraphy" ("Journal," Inst. Elec. Eng., Vol. XLIV., p. 349, 1910), in which he described an apparatus consisting of a differential air thermometer having tubular bulbs into which similar wires could be placed, and by means of which a comparison could be made of the high-frequency (H.F.) resistance R′ of a straight wire and its steady or ohmic resistance R. If two equal wires have passed through one, a steady current A and through the other a H.F. current A_1, then if these currents are adjusted until the rate of heat evolution in each case is the same we have $A^2R = A_1{}^2R'$. Certain precautions are described in the Paper for eliminating inequalities, but by means of correct reading H.F. ammeters as devised by the Author, the ratio of the resistances R′/R can be determined from the ratio of the mean square currents $A^2/A_1{}^2$.

The H.F. currents used were obtained by condenser discharges and the equiheating steady current determined by means of the differential thermometer arrangement having two equal wires in the two tubular bulbs. It is then shown that the results for straight wires agree very well with the formulæ given by Lord Kelvin and by Dr. A. Russell for the ratio R′/R for wires of different sizes and for different frequency. It is pointed out that the correction to be applied for damped oscillations as compared with persistent oscillations is at most 1 per cent. in the cases measured and that the correction for the heating effect of the condenser charging current is negligible. The case of spiral wires is then discussed. The resistance R″ of a spiral is greater than that of the same wire R′ stretched out straight. In the cases examined the ratio R″/R exceeds R′/R by about 50 to 80 per cent. A formula given by Dr. Nicholson for R″/R is then discussed, and it is shown that it does not agree with the results of observation. Experiments are also described on the H.F. resistance of wires of magnetic metals, and it is shown that in this case the observed value of R′/R can be used to determine the permeability for small H.F. magnetising forces.

DISCUSSION.

Dr. **Russell** said the verification of an abstruse mathematical formula was a notable achievement. The suggested method of getting the high-frequency permeability of steel wires was ingenious, and would be of value at low flux densities, but at high flux densities the assumption of a constant value of μ on which the formula had been obtained was not permissible. When the pitch of the helix was small and the frequency was not very high, he thought that the formula obtained by Esau, "Annalen der Physik," Jan., 1911, would be of value. Dr. Nicholson had attacked the much more difficult problem of a helix of considerable pitch. He was not prepared to say that the formula for the high-frequency resistance obtained by Dr. Nicholson was erroneous. In a straight wire the flux density was greatest round the circumference, but in a helix of small pitch it was greatest near the axis of the helix. For moderate pitches it was therefore difficult to see how the current would distribute itself. On experimental grounds, however, Dr. Fleming's criticism was quite justified, and his results would do much to encourage mathematical research.

Mr. **Duddell** drew attention to the importance of being able to predict the high-frequency resistance of spiral wires on account of their numerous applications. His own experiments had only been made on insulated wires. He found that the insulation increased the resistance owing probably to dielectric losses in the insulating material.

Dr. **Erskine-Murray** remarked that unless we stick to the definition of H.F. resistance as resistance to a uniform H.F., A.C., we shall soon be at cross purposes, since the resistance to a damped intermittent current is not the same thing, though in Dr. Fleming's particular case it does not differ by more than 1 per cent. He was interested to hear that the sources of error which he pointed out in Dr. Fleming's previous Paper on the subject do not amount to more than 1 per cent. under the conditions of the experiments. Wireless telegraphists would be grateful to Dr. Fleming for providing them with a method which will enable them to make much needed experiments, and we might look forward to obtaining a really useful knowledge of the subject in the course of two or three years.

Major O'**Meara** thought the method well worth following up, and said he hoped it would be followed up at the Post Office.

Dr. W. E. **Sumpner** observed that it did not really matter much whether a particular formula for spiral wires was correct or not. What was really important was that the tests showed Dr. Fleming's apparatus gave for the H.F. resistance of straight wires results in very close accord with established formulæ. It might, therefore, be reasonably expected to give the correct resistance for wires of all kinds, whether spirally wound or not.

Prof. E. **Wilson** drew attention to the inductive action between the two circuits which, though negligible for straight wires, might not be so when one was a spiral.

Prof. C. H. **Lees** remarked that we must depend upon experiments for knowledge of the high-frequency resistance of wires, and not upon mathematics.

Dr. J. W. **Nicholson** said the formulæ given in his Paper, referred to by Prof. Fleming, were restricted to the case in which the pitch of winding was not small. Emphasis was laid upon this fact in the introduction to his Paper, in which it was stated that Cohen's formula appeared to be quite satisfactory for all cases in which the pitch is small. Cohen's experiments strongly tended to show this, although it is necessary to notice, as Prof. Fleming has done, that these experimental tests were confined to rather low frequencies. He stated that his Paper proposed to deal with the other extreme case in which the pitch was not small, and that a connection between his formulæ and those of Cohen could not be made without a consideration of the difficult case in which the pitch is moderately small. But as, in the final statement of his results, this emphasis was not repeated, the fault of the misunderstanding was perhaps to a great extent his own. The precise restriction is that the pitch shall be of the same order of magnitude as the radius of the cylinder of winding, and in this case, the correction for spirality must be a small quantity. Since the α of his formula tends to be near $\frac{\pi}{2}$, $1-5\sin^2\alpha$ will be negative for all the cases to which the formula applies, and the correction for spirality is therefore positive, and not negative, as Prof. Fleming deduced by writing α a small quantity. The proof that spirality increases the resistance, given by Prof. Fleming in the Paper read before the Institution of Electrical Engineers, is very elegant.

It seems on consideration of the numerical magnitudes of the quantities in Prof. Fleming's experiments, that Cohen's formula ought to meet the case approximately, and that these experiments would supply a useful test of that formula for high frequencies. Prof. Fleming does not appear to have tested the formula in this way as yet, but the conditions of the experiments are somewhat close to those assumed by Cohen. In reality they are those of the intermediate cases, but α is so small that they are much nearer Cohen's conditions than to Dr. Nicholson's.

The AUTHOR, in reply, said he was glad to find that Dr. Russell substantially agrees with him that there is room for further investigation in the formula for the high-frequency resistance of spiral wires. If Dr. Nicholson's formula is valid only for spirals of large pitch, *i.e.*, greater than about 30 deg., then his formula is qualitatively right, although it has yet to be shown that it is quantitatively correct. Nevertheless the only case of practical interest is that of a tolerably close spiral for which Nicholson's formula fails. The author omitted to note that one of the assumptions made by Dr. Nicholson was that the pitch of the spiral must be large. Hence part of the force of this criticism is removed.

XI. *The Measurement of Energy Losses in Condensers Traverse, by High-Frequency Electric Oscillations.*

By J. A. **Fleming**, *M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and* G. B. **Dyke**, *B.Sc., A.M.Inst. Elec. Eng.*

Read January 27, 1910.

In apparatus for radio-telegraphy on the spark system, the oscillations are created by a discharge of a condenser, and the efficiency of the transmitting apparatus will therefore be affected by any energy losses in the condensers used. It is sometimes assumed that these energy losses are negligible, and that the chief sources of energy dissipation in the transmitter are the spark resistance and the high-frequency resistance of the circuits. This, however, is not the case, and the object of the experiments here described was to put in practice a simple method of measuring energy losses in condensers traversed by high-frequency currents. These losses may be regarded as if they were due to a resistance in series with the condenser, the dielectric being supposed to have no resistance. That is to say, we may consider that the actual condenser used is equivalent, as far as energy losses are concerned, to a condenser of the same capacity, but with perfect non-dissipative dielectric, having in series with it a resistance of some kind in which the condenser current dissipates energy. We cannot however, consider this resistance to be a constant resistance. It is a function of the condenser current, since the energy losses are not simply proportional to the mean square value of the condenser current. The assumed resistance is more analogous to the internal resistance of a voltaic cell. We can, however, state it as having a particular value for a certain current, or rather for the mean of two currents not very different in value.

After several different methods had been tried, the following plan seemed to give reliable and consistent results. The condenser to be tested is joined up in series with a circuit consisting of a copper wire of known diameter and resistance wound on a square frame so that its high-frequency resistance can be calculated, and its inductance measured. This circuit forms a non-radiative or feebly radiative circuit, hence if free oscillations are set up in this circuit they are damped out by resistance and condenser losses only. In circuit is also inserted a hot wire ammeter of the kind described by one of us (Dr. Fleming) as suitable for measuring high-frequency currents, and the circuit is interrupted also by a pair of mercury cups so that various short-wire resistances can be inserted into the circuit to vary its resistance by a known amount without sensibly altering its inductance.* This condenser circuit, which will be called the secondary circuit, is placed in contiguity to another primary circuit in which highly-damped oscillations are set up by means of an impact discharger. The primary circuits consist of a square circuit, of one or two turns of wire of the same size as the secondary circuit having in series with it one or two Leyden jars and an impact discharger.

The impact discharger used is similar in general principle to one devised by W. Peukert, but modified by Prof. Fleming.† It consists of two round discs, A_1, A_2 (*see* Fig. 1) of polished steel turned extremely true, case-hardened and ground dead flat. One of these A_2 has a hole F in the centre, the other A_1 is fixed to a shaft, B, running true in ball bearings. This shaft is carried in a frame, to which the stationary disc is fixed by adjusting screws, C, C; the two discs are insulated from each other and are placed so that their surfaces are truly parallel and separated only by a quarter of a millimetre. The frame carrying the discs is placed in a glass vessel filled with paraffin oil, and the upper disc is revolved by an electric motor at a speed of 2,000 revs. per min. The moving disc is connected to one insulated terminal, D, by a rubbing contact, E, on the shaft, and the fixed disc is connected to another insulated terminal, D′. When the upper disc revolves at a high speed it flings the oil out between the discs and fresh oil is sucked in at a hole, F, in the lower disc. There is therefore a continual circulation of the oil between the plates, and by means of adjusting screws the lower disc is placed with its surface perfectly parallel with the under surface of the revolving disc. If, then, these discs are made the discharger in a condenser circuit, the discharge takes place perfectly uniformly over the whole surface of the two discs, that is to say, it does not take place continually at one spot. Moreover, it is a dead-beat discharge. The spark is damped out of existence instantly, and although the oil becomes carbonised it is continually being renewed between the discs, and the products of decomposition do not remain between the plates.

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* *See* "Some Quantitative Measurements in connection with Radiotelegraphy," by J. A. Fleming, "Journal of the Institution of Electrical Engineers," Vol. XLIV., 1910, page 352, for a description of this hot-wire ammeter.

† For a description of Peukert's discharger, *see* "The Electrician," Vol. LXIV., p. 550, Jan. 14, 1910.

If two or more such dischargers are joined up in series, we have a very efficient impact discharger, which will run for hours by the aid of a small electric motor without attention.

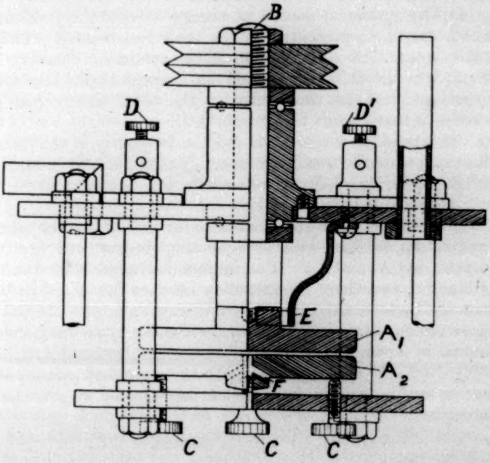

FIG. 1.

If, then, such a discharger is used in the primary circuit, the oscillations in that circuit are highly damped and practically the discharge is non-oscillatory. On the other hand, the effect on the secondary circuit is to set up in the latter its free vibrations. As there are an enormous number of sparks per second, we obtain very steady feebly damped oscillations in the secondary circuit.

Suppose, then, we measure the R.M.S. value of the current in the secondary circuit, we have four causes for the damping out of the free oscillations taking place in that circuit. These are as follows :—

(1) The high-frequency resistance R' of the square circuit, which can be calculated from the dimensions of the wire and from the frequency, which last can be experimentally determined.

(2) The high-frequency resistance of the interpolated resistance r, which is also known.

(3) The resistance of the ammeter r'.

(4) The unknown source of energy loss in the condenser, which may be represented as due to a resistance ρ, which is not constant but is a function of the condenser current. If, then, A is the R.M.S. value of this current in the secondary circuit as read on the ammeter, the total energy loss per second in that circuit is given by $A^2(R+r_1+r'+\rho)$, and if the inductance of the circuit is L and the frequency of the oscillations is n then the total decrement $\triangle$ of the circuit is equal to $10^9(R+r_1+r'+\rho)/4nL$, where $\delta=10^9\rho/4nL$, is that part of the decrement contributed by the condenser.

If, then, we alter the resistance in the secondary circuit from r_1 to r_2 we have two corresponding observed values of the current, say A_1 and A_2. If an impact discharger is used in the primary, circuit there is no reaction between the secondary and primary because the primary spark ceases almost at once, and hence the moment the secondary oscillations begin the primary circuit is open. The impact discharger communicates at every discharge the same energy to the secondary circuit, and that energy is entirely dissipated as heat, hence we must have the equation

$$A^2(R'+r_1+r'+\rho)=a\ constant=A^2R.$$

Accordingly, a curve whose ordinates are the total high-frequency resistance R and the mean square current A^2 is an equilateral hyperbola. If, however, the resistance ρ is a function of the current, then if we slightly change the added resistance r of the circuit from one value r_1 to another r_2, and take two corresponding readings of the current, we shall know the following equations :—

$$A_1^2(R'+r_1+r'+\rho)=A_2^2(R'+r_2+r'+\rho),$$

or

$$\rho=\frac{A_2^2r_2-A_1^2r_1-(A_1^2-A_2^2)(R'+r')}{A_1^2-A_2^2}.$$

The value of ρ is considered to belong to the mean value of A_1 and A_2.

In this manner we have been able to measure for various forms of condenser the value of the equivalent resistance ρ and of that part of the total decrement $\triangle$ contributed by the condenser. The first condenser used was a variable capacity oil condenser with semi-circular metal plates movable more or less in between each other by turning a handle. The oil used was a limpid vaseline oil. The results of the observations are set out in the various Tables in subsequent pages. In each case the equivalent resistance of the condenser ρ is stated as corresponding to a certain current which is the mean of two observed currents, one slightly greater and the other slightly less than the mean value by which the resistance is calculated by the above formulæ. The results show that in all cases this equivalent resistance ρ increases with the condenser current. The power W dissipated in the condenser is measured by the product of this resistance ρ and the square of the corresponding mean current A, whilst the decrement δ is the quotient of $10^9 \times$ this resistance by the quantity $4nL$, where n is the frequency and L the total inductance of the circuit. In all cases the frequency of the oscillations n for each experiment was measured by a cymometer.

In this manner we have tested an air condenser, a number of Leyden jars, a condenser made with ebonite as dielectric, and various forms of glass plate condenser, and a condenser made by Moscicki. The results are embodied in the following Tables, which will explain themselves. We have also measured the bulk of the dielectric in each case and stated the energy dissipation D in microwatts per cubic centimetre of the dielectric and also the R.M.S. value of the electric force E in the dielectric calculated from the charging current and the capacity and the geometric dimensions of the dielectric. From these figures it appears highly probable that the energy loss per cubic centimetre of the dielectric varies as a power of the electric force not far from 3 or 4.

It is evident from the Tables that the internal losses in the condensers increase with great rapidity with an increase in the potential difference between the plates, or rather with the electric force in the dielectric.

It is clear, from these observations, that even in the case of air condensers and oil condensers, energy losses are not entirely absent, whilst for certain forms of glass plate condenser this energy loss may amount to a very considerable value. It is of great importance to determine the law according to which these energy losses increase with the electric force in any dielectric so as to be able to predetermine from observations made with one charging current what the losses would be for any other charging current. The case is exactly analogous to that of the energy losses produced in iron by magnetisation. From the empirical law, discovered by Steinmetz, connecting together the energy loss per cycle of magnetisation with the maximum value of the flux density during the cycle, we can determine the energy loss due to magnetic hysteresis corresponding to any other maximum flux density and for any given material by means of a formulæ of the type, $H = \eta B_{max.}^{1\cdot6}$, where η is constant for one particular kind of iron, and $B_{max.}$ is the maximum value of the flux density during a cycle of magnetisation.

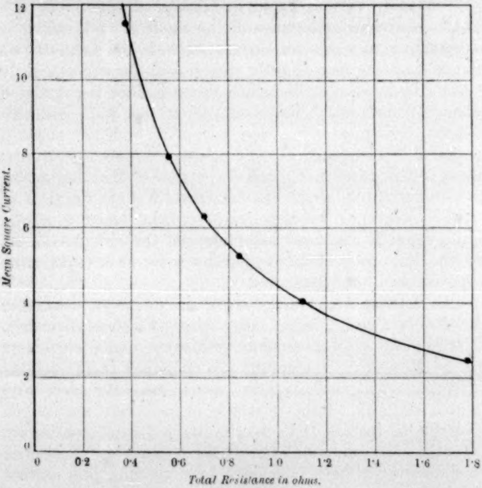

FIG. 2.

Researches conducted by Prof. R. Threlfall on the conversion of electric energy in dielectrics (*see* "Physical Review," Vol IV., p. 457; Vol. V., p. 21), have shown that the energy losses in dielectrics in the form of spheroids tested in a rotating electric field, and therefore at low frequencies, can be represented by a formula of the same type, viz., $W=aE^n$, where E is the internal electric force and W the energy dissipated and a and n are constants. Threlfall states that for most ordinary dielectrics the exponent n lies between 1·5 and 1·95, but he found that the coefficient a varies a good deal from sample to sample. Our experiments have shown that the energy loss in condensers traversed by high-frequency currents may be made to fit into a formula of a similar type within certain ranges of the electric force. There are, however, some cases in which the observed values can only be made to fit into such a formula by variation of the values of a and n over particular ranges of electric force.

The source of the energy loss in condensers inserted in circuits in which high-frequency currents are flowing, may, however, be complex. Perhaps they are partly due to true conduction losses, partly to brush discharges at the edges of the plates and partially to true dielectric losses which may be tentatively described as dielectric hysteresis.

TABLE I.

Condenser tested .... Adjustable oil condenser.
Capacity .... 0·00266 microfarad.

Total high-frequency resistance of circuit. R.	R.M.S. value of condenser current. A.	Power delivered to secondary circuit. A^2R.
0·383	3·39	4·40
0·556	2·81	4·39
0·701	2·51	4·42
0·843	2·30	4·44
1·099	2·01	4·44
1·779	1·58	4·45

The first set of experiments were made with a condenser consisting of metal plates immersed in vaseline oil. By measurements of the current A and total resistance R of the circuit as given in Table I., a series of observed values of A^2 and R were found which fit extremely well on the hyperbola $A^2R=4{\cdot}42$.

This hyperbola is represented by the firm line in Fig. 2, and the dots on it represent the observations made in Table I.

The observations recorded in Table IA show that for this condenser the equivalent resistance is small and nearly constant over the range of electric force used.

TABLE IA.

Condenser tested	Adjustable oil Condenser No. II.
Capacity	0·00266 microfarad.
Frequency	$1·11 \times 10^6$.
Nature of dielectric	Vaseline oil.
Surface of dielectric	3,150 sq. cms
Thickness of dielectric	0·238 cm.
Volume of dielectric	750 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·10	0·052	0·500	0·00156	667	2·34
2·61	0·050	0·341	0·00150	455	1·97
2·40	0·060	0·346	0·00180	461	1·82
2·15	0·057	0·263	0·00171	351	1·63
1·80	0·060	0·194	0·00180	259	1·36

The observed power loss D in this condenser is related to the corresponding electric force E in the manner expressed by the formula $D=121E^{2·15}$, which fits in well with the above observations.

The observations in Tables IB. and IC. represent the results for the same condenser but adjusted for different capacities.

TABLE IB.

Condenser tested	Adjustable oil Condenser No. II.
Capacity	0·00140 microfarad.
Frequency	$1·55 \times 10^6$.
Nature of dielectric	Vaseline oil.
Surface of Dielectric.......... ..	1,575 sq. cms.
Thickness of dielectric	0·238 cm.
Volume of dielectric	375 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·20	0·119	1·218	0·00256	3,250	3·29
2·83	0·100	0·801	0·00215	2,140	2·91
2·58	0·104	0·692	0·00224	1,840	2·65
2·34	0·111	0·608	0·00239	1,620	2·40
1·99	0·123	0·487	0·00264	1,300	2·04

The above observations agree with the formula,

$$D=243·2E^{2·15}.$$

TABLE Ic.

Condenser tested	Adjustable oil Condenser No. II.
Capacity	0·00076 microfarad.
Frequency	$2{\cdot}14\times10^6$.
Nature of dielectric	Vaseline oil.
Surface of dielectric	788 sq. cms.
Thickness of Dielectric	0·238 cm.
Volume of dielectric	188 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electro-static units. E.
3·16	0·245	2·45	0·00382	13,020	4·34
2·87	0·247	2·04	0·00385	10,850	3·94
2·67	0·227	1·62	0·00354	8,630	3·66
2·45	0·247	1·48	0·00385	7,880	3·36
2·10	0·227	1·00	0·00354	5,330	2·88

The above observations agree with the formula,

$$D=550E^{2\cdot 15}.$$

The observations recorded in Tables ID., IE., and IF. refer to another vaseline oil condenser of the same type, taken for various capacities, and the exponential formula beneath each Table is that which best expresses the relation of loss to force in each case. The oils were not from the same sample, which probably accounts for the difference in the value of the exponents.

TABLE ID.

Condenser tested	Adjustable oil Condenser No. I.
Capacity	0·00271 microfarad.
Frequency	$1{\cdot}12\times10^6$.
Nature of dielectric	Vaseline oil.
Surface of dielectric	3,150 sq. cms.
Thickness of dielectric	0·238 cm.
Volume of dielectric	750 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electro-static units. E.
3·14	0·072	0·710	0·00211	950	2·31
2·71	0·054	0·396	0·00161	530	1·99
2·45	0·036	0·216	0·00107	290	1·80
2·18	0·028	0·133	0·00083	180	1·60
1·81	0·028	0·092	0·00083	120	1·33

$$D=71E^{2\cdot 61}.$$

TABLE IE.

Condenser tested	Adjustable oil Condenser No. I.
Capacity	0·00141 microfarad.
Frequency	$1{\cdot}54\times10^{6}$.
Nature of dielectric	Vaseline oil.
Surface of dielectric	1,575 sq. cm.
Thickness of dielectric	0·238 cm.
Volume of dielectric	375 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·15	0·121	1·200	0·00262	3,200	3·24
2·78	0·122	0·943	0·00264	2,510	2·86
2·55	0·107	0·695	0·00232	1,850	2·62
2·30	0·096	0·508	0·00208	1,350	2·36
1·95	0·105	0·399	0·00227	1,060	2·00

$$D=155E^{2{\cdot}61}.$$

TABLE IF.

Condenser tested	Adjustable oil Condenser No. I.
Capacity	0·00076 microfarad.
Frequency	$2{\cdot}11\times10.^{6}$
Nature of dielectric	Vaseline oil.
Surface of dielectric	788 sq. cms.
Thickness of dielectric	0·238 cm.
Volume of dielectric	188 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·26	0·264	2·80	0·00417	14,900	4·59
2·96	0·250	2·19	0·00395	11,600	4·16
2·76	0·209	1·59	0·00330	8,470	3·88
2·53	0·210	1·34	0·00332	7,140	3·56
2·18	0·212	1·01	0·00335	5,380	3·06

$$D=275E^{2{\cdot}61}.$$

These formulæ are all of the type $D=XE^{Y}$. It may be noticed that for the same oil and same condenser the exponent **Y** remains the same, but the multiplying factor **X** varies nearly inversely as the capacity or directly as the current density.

The observations recorded in Table II. for a glass plate condenser show exactly how the observations were taken in each case and treated so as to obtain the effective resistance ρ of the condenser for a certain mean current A.

It will be seen that this equivalent condenser resistance is a function of the condenser current and increases with it. Hence that part of the total decrement due to the condenser losses increases with the condensers current. The bulk of the dielectric, its thickness, and the frequency were measured in each case as well as the inductance of the condenser circuit.

Table II.

Example of Calculation.

Condenser tested .... Glass plate condenser in oil.
Capacity .... 0·00130 microfarad.
Frequency .... $1{\cdot}68 \times 10^6$.
Inductance of circuit ...... 6,920 cms.

The circuit made up of No. 14 S.W.G. copper wire (diameter $=0{\cdot}203$ cm.), using Dr. A. Russell's formula for the ratio of the high-frequency resistance to the steadycurrent resistance. In the case of the circuit used this ratio is 10·29, and hence since the steady current resistance of circuit $=0{\cdot}0350$ ohm, the high-frequency resistance $R'=10{\cdot}29\times0{\cdot}0350$ $=0{\cdot}360$ ohm.

The ammeter used was constructed of copper wire, and hence its resistance increased with the current passing (*see* Table), but this variation was determined and allowed for.

High-frequency resistances in ohms.				R.M.S. value of condenser current in amperes A_1.	A_1^2.	A_1^2R.	Difference of values of AR.	Difference of values of A^2.	Effective resistance of condenser in ohms. ρ	Mean value of R.M.S. current in amperes A.
Interpolated resistance r.	Ammeter resistance r'	Circuit resistance R'.	Sum. R.							
0	0·047	0·360	0·407	3·47	12·04	4·900				
							1·238	1·51	0·820	3·36
0·177	0·046	0·360	0·583	3·245	10·53	6·138				
							0·786	0·98	0·803	3·17
0·320	0·045	0·360	0·725	3·09	9·55	6·924				
							0·627	0·80	0·785	3·02
0·459	0·014	0·360	0·863	2·957	8·75	7·551				
							0·916	1·19	0·770	2·85
0·717	0·043	0·360	1·120	2·75	7·56	8·467				
							1·427	2·06	0·694	2·55
1·398	0·041	0·360	1·799	2·345	5·50	9·894				

The observations recorded in Table III. were made in the above described manner, using a condenser made with dielectric of sheet ebonite and the whole immersed in insulating oil to prevent brush discharges. The results of the measurements show that the equivalent condenser resistance ρ increases with the condenser current and that the condenser losses in microwatts per cubic centimetre D vary with the electrostatic force E in the dielectric, according to the law,

$$D=1{,}780E^{2{\cdot}87}.$$

Tables IV. and V. show measurements made with similar condensers of increasing capacity, but the factor X seems to decrease with the mean electric force.

TABLE III.

Condenser tested	Ebonite in oil.
Capacity	0·00084 microfarad.
Frequency	$2{\cdot}00\times10^6$.
Nature of dielectric	Ebonite.
Surface of Dielectric	1,050 sq. cms.
Thickness of dielectric	0·3 cm.
Volume of dielectric	315 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
2·73	1·59	11·85	0·0265	37,700	2·87
2·63	1·52	10·52	0·0253	33,400	2·77
2·55	1·43	9·29	0·0238	29,600	2·69
2·45	1·39	8·34	0·0232	26,400	2·58
2·25	1·37	6·93	0·0229	22,000	2·37

The condenser losses in microwatts per cubic centimetre are represented by the formula,

$$D=1{,}780E^{2{\cdot}87}.$$

TABLE IV.

Condenser tested	Ebonite in oil.
Capacity	0·00170 microfarad.
Frequency	$1{\cdot}41\times10^6$.
Nature of dielectric	Ebonite.
Surface of dielectric	2,100 sq. cms.
Thickness of dielectric	0·3 cm.
Volume of dielectric	630 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·24	0·850	8·92	0·0201	14,100	2·39
3·05	0·738	6·86	0·0174	10,900	2·25
2·90	0·715	6·01	0·0169	9,550	2·14
2·73	0·704	5·25	0·0166	8,350	2·02
2·41	0·657	3·82	0·0155	6,060	1·78

The results agree with the formula,

$$D=1{,}120E^{2{\cdot}87}.$$

TABLE V.

Condenser tested .... Ebonite in oil.
Capacity .... 0·00258 microfarad.
Frequency .... $1·14\times10^6$.
Nature of dielectric .... Ebonite.
Surface of dielectric .... 3,150 sq. cms.
Thickness of dielectric .... 0·3 cm.
Volume of dielectric .... 945 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·25	0·659	6·96	0·0193	7,380	1·96
3·02	0·577	5·26	0·0169	5,580	1·82
2·85	0·512	4·16	0·0150	4,400	1·72
2·65	0·504	3·54	0·0147	3,750	1·60
2·32	0·458	2·54	0·0134	2,690	1·40

The results fit in with the formula,

$$D=1{,}000E^{2·87}.$$

Table VI. exhibits the results of measurements made with an air condenser, and show that though the energy losses are small they are by no means zero even in air condensers; but are immensely less than the losses in glass or ebonite dielectrics for equal volumes of dielectric but not much less for equal values of the electric force.

TABLE VI.

Condenser tested .... Air Condenser with zinc plates.
Capacity...................... 0·001093 microfarad.
Frequency .... $1·63\times10^6$.
Nature of dielectric ...... Air.
Surface of dielectric...... 58,800 sq. cms.
Thickness of dielectric... 5·0 cms.
Volume of dielectric...... 294,000 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·22	0·049	0·508	0·00086	1·73	0·192
2·84	0·046	0·371	0·00081	1·26	0·169
2·60	0·039	0·264	0·00069	0·90	0·155
2·35	0·031	0·171	0·00055	0·58	0·140
1·99	0·026	0·103	0·00046	0·35	0·119

The results can be represented by the formula,

$$D=590E^{3·5}.$$

Tables VII., VIII., and IX. give the results of measurements made with Leyden jars, such as are used in radio-telegraphy. The jars used in the experiments in Tables VII. and VIII. agree in giving a formula for the loss in terms of the electrostatic force very nearly the same, but the jar used in the experiments in Table IX. gave results which fit in best with a formula with larger value for X and lesser value for the exponent Y.

TABLE VII.

Condenser tested	Leyden jar.
Capacity	0·00200 microfarad.
Frequency	$1{\cdot}30\times10^6$.
Nature of dielectric	Glass.
Surface of dielectric	630 sq. cms.
Thickness of dielectric	0·20 cm.
Volume of dielectric	126 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·24	0·140	1·47	0·00359	11,700	3·31
2·83	0·118	0·945	0·00303	7,500	2·89
2·65	0·095	0·667	0·00244	5,300	2·71
2·34	0·074	0·405	0·00190	3,220	2·39
1·96	0·045	0·173	0·00115	1,370	2·00

The results agree with the formula $D=77E^{4\cdot24}$.

TABLE VIII.

Condenser tested	Leyden jar.
Capacity	0·00211 microfarad.
Frequency	$1{\cdot}27\times10^6$.
Nature of dielectric	Glass.
Surface of dielectric	650 sq. cms.
Thickness of dielectric	0·22 cm.
Volume of dielectric	143 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·19	0·123	1·25	0·00323	8,750	2·88
2·79	0·084	0·653	0·00220	4,560	2·52
2·54	0·079	0·510	0·00207	3,570	2·29
2·29	0·053	0·278	0·00139	1,940	2·06
1·91	0·027	0·098	0·00071	690	1·72

The results can be represented by the formula,

$$D=95E^{4\cdot24}.$$

TABLE IX.

Condenser tested	Leyden jar.
Capacity	0·00204 microfarad.
Frequency	$1·29 \times 10^6$.
Nature of dielectric	Glass.
Surface of dielectric	634 sq. cms.
Thickness of dielectric	0·27 cm.
Volume of dielectric	171 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·19	0·117	1·190	0·00302	6,970	2·39
2·80	0·103	0·808	0·00266	4,730	2·10
2·55	0·096	0·624	0·00248	3,650	1·91
2·30	0·084	0·444	0·00217	2,600	1·72
1·93	0·080	0·298	0·00207	1,740	1·45

The results fit in with the formula $D=610E^{2·75}$.

TABLE X.

Condenser tested	Glass plate condenser in oil.
Capacity	0·00130 microfarad.
Frequency	$1·68 \times 10^6$.
Nature of dielectric	Glass.
Surface of dielectric	309 sq. cms.
Thickness of dielectric	0·17 cm.
Volume of dielectric	52·5 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
3·36	0·820	9·25	0·0176	176,000	4·81
3·17	0·803	8·06	0·0173	154,000	4·54
3·02	0·785	7·15	0·0169	136,000	4·32
2·85	0·770	6·25	0·0166	119,000	4·08
2·55	0·694	4·50	0·0149	85,800	3·65
2·31	0·678	3·62	0·0146	69,000	3·30
2·16	0·676	3·16	0·0145	60,200	3·09
2·05	0·638	2·68	0·0137	51,100	2·94
1·93	0·664	2·47	0·0143	47,100	2·76
1·71	0·623	1·82	0·0134	34,700	2·45
1·17	0·550	0·754	0·0118	14,400	1·67
1·10	0·498	0·604	0·0107	11,500	1·57
1·04	0·458	0·495	0·0099	9,440	1·49
0·97	0·446	0·419	0·0096	7,990	1·39
0·85	0·370	0·266	0·0080	5,060	1·22
0·67	0·434	0·195	0·0093	3,720	0·96
0·62	0·406	0·154	0·0087	2,930	0·89
0·59	0·411	0·144	0·0088	2,740	0·84
0·55	0·430	0·129	0·0093	2,460	0·79
0·48	0·280	0·064	0·0060	1,220	0·69

The above observations agree with the formula $D=3,720E^{2·42}$ well down to $E=1·4$, then not so well.

The observations in Table X. concern the losses in a glass plate condenser and agree well with the formula $D=3{,}720E^{2·42}$, but those in Table XI. for another glass plate condenser with much thicker plates and those in Table XII. taken with a Moscicki condenser give results which will not fit in with any single formula of this type, but taken in stages for various ranges of E can be represented by several different formulæ of this form.

TABLE XI.

Condenser tested .... Glass plate condenser in oil.
Capacity .... 0·0139 microfarad.
Frequency .... $0·51 \times 10^6$.
Nature of dielectric .... Glass.
Surface of dielectric .... 12,080 sq. cms.
Thickness of dielectric ...... 0·51 cm.
Volume of dielectric .... 6,160 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.
9·90	0·357	35·0	0·0250	5,700	1·46
9·60	0·306	28·2	0·0215	4,580	1·41
9·30	0·286	24·7	0·0200	4,010	1·37
9·02	0·274	22·3	0·0192	3,620	1·33
8·39	0·256	18·0	0·0180	2,920	1·23
7·28	0·171	9·06	0·0120	1,470	1·07
6·97	0·141	6·85	0·0099	1,110	1·02
6·67	0·126	5·61	0·0088	911	0·98
6·41	0·117	4·81	0·0082	781	0·94
5·85	0·110	3·76	0·0077	610	0·86
3·14	0·079	0·779	0·0056	126	0·46
2·67	0·070	0·499	0·0049	81	0·39
2·39	0·062	0·354	0·0044	57	0·35
2·12	0·045	0·202	0·0032	33	0·31
1·75	0·016	0·049	0·0011	8	0·26

From $E=1·45$ to $0·9$. $D=1{,}200E^{4·41}$ points fit very well.
$E<0·9$. $D=903E^{2·61}$. points fit badly.

It is clear, therefore, that the energy losses in air and oil condensers made with certain oils are, relatively speaking, very small, but may yet amount to something of the order of several thousand microwatts per cubic centimetre for electrostatic forces of the order of 2·0 electrostatic units or 600 volts per centimetre. They are also determined by the current density. In the case of condensers made with glass or ebonite dielec-

TABLE XII.

Condenser tested	Moscicki condenser.
Capacity	0·004067 microfarad.
Frequency	$0·88 \times 10^6$.
Nature of dielectric	Glass.
Surface of dielectric	930 sq. cms.
Thickness of dielectric	0·15 cm. (very roughly).
Volume of dielectric	139 cub. cms.

R.M.S. value of condenser current in amperes. A.	Effective resistance of condenser in ohms. ρ	Power dissipated in condenser in watts. W.	Decrement of condenser. δ	Power loss in condenser in microwatts per c.c. D.	R.M.S. value of electric force in dielectric in electrostatic units. E.	
8·47	0·760	54·7	0·0270	394,000	8·38	$D=1,000E^{2·8}$.
8·32	0·729	50·4	0·0259	362,000	8·23	
8·18	0·742	49·5	0·0264	356,000	8·09	
8·04	0·710	45·9	0·0252	330,000	7·95	
7·60	0·684	40·4	0·0242	291,000	7·60	
7·58	0·680	39·0	0·0242	281,000	7·50	
7·44	0·649	35·9	0·0230	258,000	7·35	$D=7·9E^{5·2}$.
7·30	0·614	32·7	0·0218	235,000	7·22	
7·17	0·585	30·1	0·0208	217,000	7·10	
6·78	0·371	17·0	0·0132	122,300	6·71	
6·30	0·386	15·30	0·0137	110,000	6·24	
6·13	0·344	12·91	0·0122	92,900	6·07	
5·97	0·327	11·64	0·0116	83,800	5·91	
5·82	0·304	10·29	0·0108	74,000	5·76	
5·44	0·234	6·92	0·0083	49,800	5·39	
3·15	0·262	2·600	0·0093	18,700	3·12	
2·82	0·186	1·479	0·0066	10,600	2·79	$D=660E^{2·8}$.
2·59	0·163	1·093	0·0058	7,900	2·56	
2·35	0·149	0·823	0·0053	5,900	2·32	
1·99	0·138	0·546	0·0049	3,900	1·97	

trics these internal energy losses may become very large for electrostatic forces attaining a value not even larger than 10, and hence contributing greatly to raise the decrement in any oscillatory circuits of which these condensers form part. Different kinds of glass seem to vary very much in this respect, and this points to the great importance of testing Leyden jars and glass plate condensers intended to be used as radiotelegraphic transmitters, especially for dielectric loss as well as for capacity. With the arrangements above described this can be most easily and accurately carried out, and that glass may be selected which is best for the purpose. The experiments also show that for experimental work condensers made with oil as dielectric are best, whilst for large scale condensers air as a dielectric gives a small energy loss, provided the electrostatic force is not raised to a value at which brush discharges begin to produce sensible ionisation of the air. If this is the case the internal losses may become considerable, even for air condensers. The moral to be drawn from the above described observations is that in the case of condensers used in Radiotelegraphy the current density in the condenser should be kept as small as possible to reduce the dielectric losses. As regards the sources of these energy losses in solid or liquid dielectrics some part of the loss is probably due to actual conduction. Some part may be due to brush discharges and the remainder to dielectric losses, which are generally referred to as dielectric hysteresis, though this term has never been accurately defined. In a Paper on "The Conversion of Electric Energy in Dielectrics," *see* Physical Review, Vol. IV., May, June; Vol. V., July, August, 1897, Prof. R. Threlfall discusses very fully the sources of energy loss in dielectrics, and found that when ellipsoids of dielectrics were suspended in a revolving electric field and the dielectric hysteresis thus measured the energy absorption could be expressed in terms of the internal electric force by an exponential formula such as that we have used. The formula, however, in itself conveys no physical meaning, but is merely a convenient method of recording the observations.

ABSTRACT.

In this Paper an arrangement of apparatus is described for the purpose of measuring the internal energy losses in condensers traversed by high frequency (H.F.) currents. It is shown that these energy losses in condensers may be considered as if they were due to a resistance loss in a hypothetical resistance in series with the condenser, the condenser itself being supposed to have a perfect non-dissipative dielectric of the same dielectric constant. This hypothetical resistance is not constant, but is a function of the condenser current. The experiments were conducted by the use of a special form of impact dischargers comprising two flat plates immersed in oil, one stationary and the other revolving at a high speed. This discharger was placed in series with a primary circuit and condenser, and H.F. oscillations were set up in the primary having any desired frequency. A secondary circuit loosely coupled consisted of a wire whose H.F. resistance could be determined, the condenser to be examined and a hot-wire ammeter and variable resistances. The measurements consisted in observing the reading of the ammeter, and then changing the current created in the secondary circuit by a small amount by adding a known resistance which altered the decrement of the circuit, but not its inductance. From these readings an equation is obtained for the hypothetical condenser resistance. It is shown that the product of

the square of the secondary current A^2 and the total resistance R of the secondary circuit is constant, and hence that the unknown condenser resistance ρ can be found from observations of the change in A^2 when an additional resistance r_1 is interpolated in the condenser circuit. The energy loss in the condenser is then $A^2\rho$ watts. Condensers with various dielectrics, air, oil, glass and ebonite, were examined, and the dielectric energy losses D are stated in micro watts per cubic centimetre of dielectric for given values of the electric force E. It is shown thus D can be expressed as a function of E in the form $D = XE^Y$, where X is a constant depending on the current density, and Y is a constant depending on the nature of the dielectric. For oil and air these power losses are relatively small, but for glass and ebonite large. The necessity for measuring these losses in the case of radiotelegraphic condensers is emphasised.

[The discussion on this paper was combined with that on the following paper on "Resonance Curves," by the same Authors.]

XII. *Some Resonance Curves taken with Impact and Spark Ball-Dischargers.* *By* J. A. FLEMING, *M.A.*, *D.Sc.*, *F.R.S.* *and* G. B. DYKE, *B.Sc.*, *A.M.Inst.E.E.*

READ JANUARY 27, 1910.

IN the course of the experiments on the energy losses in condensers traversed by high-frequency currents described in the previous Paper,* we had occasion to take many measurements with the cymometer of the frequency and inductance of the secondary circuit used in which oscillations were created by a primary circuit used both with the impact and with the ordinary spark-ball discharger. It then became an easy matter to take sufficient observations to delineate a complete resonance curve in each case. As these curves give a very interesting insight into the operations taking place in the secondary circuit and as they confirm the ordinary theory of the oscillation transformer, and, moreover, exhibit the striking difference between the action of an impact and of an ordinary spark-ball discharger, we have delineated for each such case a family of resonance curves taken over a certain range of frequency and with various degrees of coupling. The arrangements for experiments were just as described in the previous Paper, only the secondary circuit was moved to or from the primary to vary the coupling and the cymometer circuit was in all cases loosely coupled to the secondary circuit. The resistance of the secondary circuit was varied so as to give in one case a small decrement of about 0·01 per semi-period and in the other case a much larger decrement of 0·09 per semi-period. The spark-gap was 1 mm. in length and acted upon by a steady blast of air to produce uniform discharges.

The current in the cymometer circuit was measured by a hot-wire thermoelectric ammeter. In the diagrams given the vertical ordinates represent this cymometer current in milliamperes. The decrement of the cymometer itself was about 0·03. The impact discharger used was that described in the previous Paper on "Energy Losses in Condensers traversed by High-Frequency Currents" (*loc. cit.*).

The curves in Fig. 1 delineate the resonance curves taken with the ordinary spark-gap and with such resistance in the secondary circuit as to give a semi-period decrement of 0·01. The different curves correspond to different degrees of coupling *k* of the primary and secondary circuits, as shown in the diagram. When the coupling is about 33 per cent. the resonance curve 1 shows three well-marked maxima or humps. The frequency corresponding to the middle hump is the natural free period frequency of the secondary circuit. The frequencies of the other two maxima on either side correspond to the two oscillations which are created by the reaction between the primary and secondary circuits.

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* "The Measurement of Energy Losses in Condensers traversed by High-Frequency Electric Oscillations." By J. A. Fleming and G. B. Dyke.

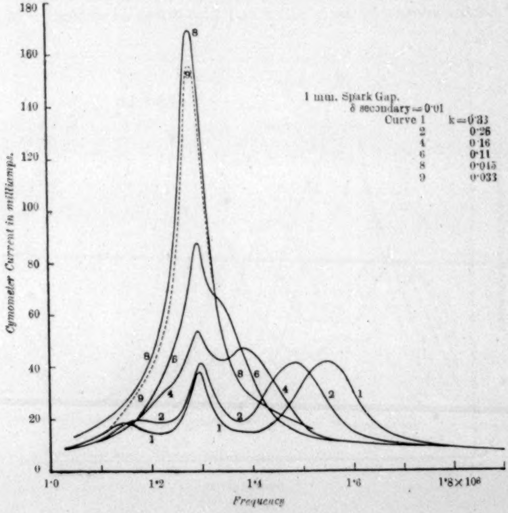

FIG. 1.

These three frequencies have such values that the square of the free frequency is the mean of the squares of the other two frequencies. This confirms and agrees with theory; also the calculated value of *k* agrees fairly well with that deduced from the two frequencies. As the coupling is reduced respectively to 26 per cent. and 16 per cent., we have the resonance curves 2 and

4 from which it is seen that the amplitudes of the two period oscillations are decreasing and the free oscillation survives. When the coupling is reduced to 11 per cent. there remains only a very slight indication of a forced oscillation. If the coupling is weakened still more we have only the free oscillation left and the secondary current comes to a maximum for a coupling of about 5 per cent. Beyond that weakening the coupling decreases the maximum current in the secondary circuit. If we take off the values of the maximum cymometer currents in each curve and plot these as ordinates of another curve to abscissæ representing the coupling, as done in Fig. 5, we have a curve (the upper dotted curve marked "spark") which shows us how the current in the secondary circuit varies with the coupling between the secondary and the primary. It has a well-marked maximum value for about 5 per cent. coupling.

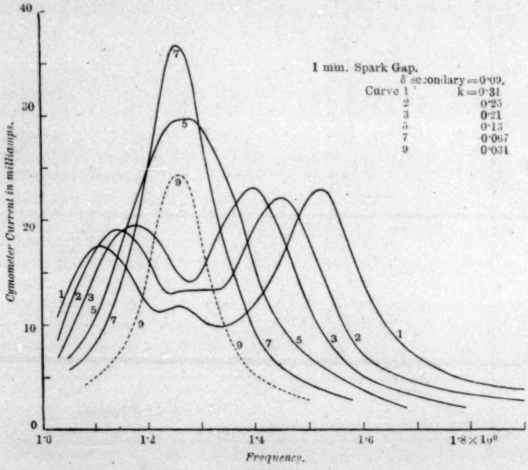

FIG. 2.

The family of resonance curves in Fig. 2 is taken with the same spark-ball discharger in the primary circuit but with resistance added to the secondary circuit to increase its semi-period decrement to 0·09. It will be seen that for a coupling of 31 per cent. there is a trace of a free vibration, but the predominant oscillations in the secondary are the two-period oscillations. As the coupling is weakened the free oscillation makes its appearance, but it is not until the coupling is weakened to 13 per cent. that the free oscillation alone survives.

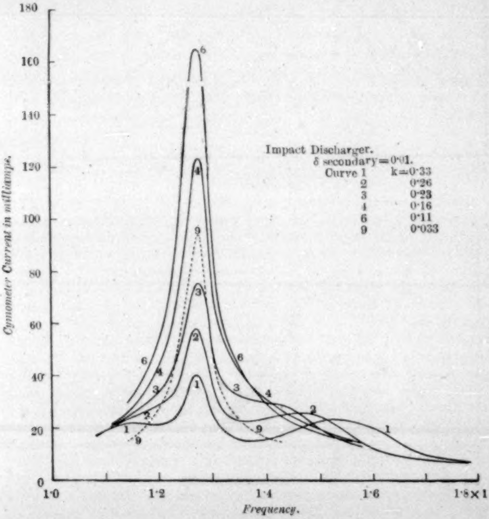

FIG. 3.

When the coupling is reduced to about 7 per cent. we have the maximum currents in the secondary circuit.

These observations were then repeated, using the impact discharger, and the same general phenomena are observed, as shown by the curves in Figs. 3 and 4, with the difference that for a secondary decrement of 0·01 the free secondary oscillation makes its appearance when the coupling is as strong as 25 per cent., and when the secondary decrement is 0·09 the free secondary oscillation alone appears even when the coupling is 20 per cent.

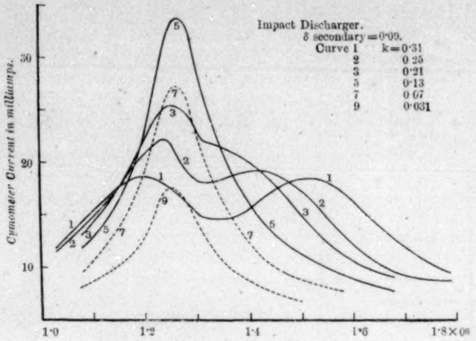
Impact Discharger.
δ secondary = 0·09.
Curve 1 k = 0·31
2 0·25
3 0·21
5 0·13
7 0·07
9 0·031
Cymometer Current in milliamps.
30
20
10
1·0
1·2
1·4
1·6
1·8 × 10⁶
Frequency.
1
2
3
5
7
9

FIG. 4.

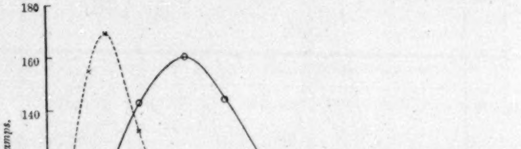
Maximum Cymometer Current in milliamps.
180
160
140
120
100
80
60
40
20
0
0
·05
·10
·15
·20
·25
·30
·35
Coefficient of Coupling.
Impact
Spark
Secondary δ = 0·01
Spark
Impact
Secondary δ = 0·09

FIG. 5.

The firm line curves in Fig. 5 represent the variation of the secondary current as the coupling between primary and secondary varies. The advantages of the impact dischargers may therefore be summed up by saying that with it we can have closer coupling between the primary and secondary circuit and yet retain the advantage of having no other oscillations set up in the secondary circuit than its natural free oscillation. This, however, omits all consideration of the number of the discharges and of the efficiency of the transmitter as a whole. Closeness of coupling always is an advantage in increasing plant efficiency, provided we can secure it without setting up forced oscillations in the secondary which annihilate the sharp peak of the resonance curve and therefore render sharp tuning between a receiving station and that particular transmitter impossible.

ABSTRACT.

In the course of the experiments described in the previous Paper on the measurement of energy losses in condensers a large number of measurements had to be made with the cymometer of the frequency of oscillations in, and the inductance of, the secondary or condenser circuit. It was then an easy matter to draw complete resonance curves in each case, and this has accordingly been done with both the impac and spark ball dischargers in the primary circuit and for various resistances in the secondary circuit. The results are interesting as showing exactly what takes place in each case in the primary circuit. If we are using the spark ball discharger, and if the primary and secondary circuits are coupled with various degrees of coupling, then, for any close coupling, we find on the resonace curve three peaks which correspond, respectively, as regards frequency, with the free oscillation period of the secondary and with the two-period oscillations set up by the reaction of the secondary upon the primary circuit. As the coupling is weakened the double-period oscillations die out, and only the free oscillation of the secondary survives. There is always a certain coupling, not far from 10 per cent., which gives the maximum current in the secondary circuit in the form of a free oscillation. If the secondary circuit is more highly damped, then the two-period oscillations are more strongly marked, and the maximum free period oscillation has a lesser maximum value.

If we are using an impact discharger the double-period oscillations are only apparent when the coupling exceeds about 30 per cent., and die away with a very little reduction in the coupling, leaving the predominant free secondary oscillation as the survivor. These curves show how very quickly the primary spark is quenched when using the impact discharger. If the maximum secondary current is set up as ordinates in terms of the coupling as abscissa we obtain curves which rise up quickly to a maximum value and fall again, and which indicate that the maximum value of the secondary current is determined both by the coupling and the secondary decrements.

DISCUSSION.

Mr. A. **Campbell** remarked that the want of constancy of the effective resistance as the current is changed seemed to be out of agreement with the results of other recent observers. Various condensers have been tested at the National Physical Laboratory at frequencies of 800 and 2,000 ∾ per second by Carey Foster's method. No divergence from the square law is found from 0·5 to 6 volts. It would be surprising if the square law did not hold at 500,000 to 1,000,000 ∾ per second. The formula employed by Dr. Fleming to give ρ can scarcely be expected to give accurate results, since so much depends upon the small difference A_1-A_2. Monasch ("Electrician," June and July, 1907) has shown most conclusively that for frequencies about 80 ∾ per second the losses in glass and ebonite condensers (and also in cables) were exactly proportional to the square of the effective voltage between 1,000 and 8,000 volts. He has pointed out that the heating of the dielectric by the power spent in it very often appears to cause variation from the square law. Stöckly ("La Lumière Electrique," July, 1909) has recently found that the decrement of a Moscicki condenser rose from 0·0165 at 10°C. to 0·021 at 40°C.

Mr. W. **Duddell** expressed surprise that with low potential gradients the energy loss was not proportional to V^2. Increase of the losses in a rather peculiar way with the frequency—not necessarily proportional—was to be expected. He cited his experiments made with high frequencies on an aerial at Bushy. The easiest method of finding the losses was to put in a resistance to bring the current down to half value. This method was justified with an air condenser.

He remarked that he thought it was the *voltage* that was induced in the secondary that was fixed, and not the amount of *energy* that was transferred.

He pointed out that we were never certain as to how much energy was lost in each part of the secondary circuit, and drew attention to the importance of knowing just where all the energy losses took place, as then we could soon begin to make our apparatus efficient.

Mr. E. H. **Rayner** suggested that a calculation of the power factors of the condensers would have given results which afforded considerably more information at a glance. If the losses varied as the square of the voltage, since they are small, the power factor would be constant at all voltages; if the losses increased faster than the square of the voltage the power factor would rise; if at a slower rate it would fall. The experimental results were not accurate enough to determine the constants of the formula employed by the authors. He had analysed the results given in the Paper, and found that some cases showed a diminishing power factor, giving an index less than 2, while the authors had given 2·15. He drew attention to some results which he had obtained on a gutta-percha cable showing a power factor practically constant from 10 volts to 4,000, demonstrating an exceedingly close approximation to the square law over a very great range of electric force.

Mr. F. **Jacob** remarked he had been for some time at Messrs. Siemens Bros. & Co.'s Cable Works, testing the leakance of gutta-percha cores and other dielectrics, or in other words the effective resistance of condensers, not certainly at the extra high periodicities of Prof. Fleming's most interesting and important results, but at such frequencies as obtained in telephonic transmission, say from 750 ∾ to 2,000 ∾. Their mode of testing was the bridge method, and the results obtained were correct within about 1 per cent. Two adjacent arms of the bridge are occupied by two equal resistances, the other two arms containing respectively the condenser K whose leakage resistance S is to be tested, and a variable condenser C, whose leakage is zero, in series with an adjustable resistance R. An alternating current and a telephone were employed. When balance is established

$$R-\frac{1}{ipC}=\frac{1}{S-ipK}.$$

Simplifying, and neglecting S^2 in comparison with p^2K^2 we find $K=C$ and $S/K=p^2RC$.

Hundreds of determinations on different materials show that for a given material and frequency S/K is a constant, and within ordinary electrical pressures independent of the same, and in some instances in direct proportion to the periodicity, but in others a little higher. His pressures corresponded very nearly to those given by Prof. Fleming's experiments.

S/K for air by his experiments was 0, and also for a mica condenser if of special construction.

In connection with Mr. Rayner's remarks, he stated that at 750 ∼ the value of S/K for the gutta-percha core of the loaded channel telephone cable was 114, and at 2,000 ∼, 0·320.

He hoped Prof. Fleming would apply his thermometric differential detector in a similar way, and thus obtain more directly the effective resistances of the condensers he had recently determined by comparison with an air condenser for which his experiments showed the effective resistance to be very small.

He had, on the assumption that the value S/K is proportional to the frequency, reduced Prof. Fleming's results to Messrs. Siemens standard form of S/K. He found no variation with electrical pressure. Below are excerpted values at the different frequencies comparing pairs which are at the same pressure.

Ebonite.

E.	C.	R.	10^6 ∼	S/K.	C 2,000 ∼
2·37	0·00084	1·37	2·0	182,500	182
2·39	0·00170	0·850	1·41	114,600	163
2·02	0·00170	0·704	1·41	94,900	135
1·96	0·00258	0·659	1·14	87,700	154
1·78	0·00170	0·657	1·41	88,600	126
1·82	0·00258	0·577	1·14	76,800	135

The last column of values reduced to 2,000 ∼ shows three values nearly identical and three differing, but no doubt further experiments will elucidate the cause.

At 2,000 ∼ for the best ebonite we get a value of 71, and at 800 of 29, which is practically proportional to those periodicities, but there are very different qualities of ebonite, and as there is oil also to take into account, and he did not know what part of the capacity is due to the two dielectrics in question.

The resistances found by Prof. Fleming are anyhow the effective values of these condensers, though we may not be able to separate the portion that is due to the dielectric alone. He found that the ordinary values of dielectric resistance measured after a minute, give no indication as to value of S/K.

Mr. G. L. **Addenbrooke** said his knowledge of high-frequency currents was not enough to enable him to speak authoritatively as to the accuracy of Dr. Fleming's method, but he had found that the form of the wave was of considerable importance, especially where the ammeter reading was relied on at all. Nevertheless he could support Dr. Fleming and Mr. Dyke in one point. He had himself found losses in air condensers after they had been in use for a time, and the loss increased until they might have a power factor of 1 per cent., that is in cases where the plates were about ¼ in. apart. He had intended calling attention to this before seeing Dr. Fleming's Paper.

Next as to the experiment in Dr. Fleming's formula where heating was not sensible, he had always found the losses increased very nearly as the square of the current, but in a Paper sent into the Institution of Electrical Engineers some time since, the publication of which he had kept back, he had shown that excessive and long continued pressure might cause heating and greatly increase the losses, and more recently he had found the converse to be the case, that the losses were considerably increased by heating the dielectric, though in different degrees in different dielectrics.

Dr. W. H. **Eccles**, referring to the Paper giving resonance curves got from a secondary circuit excited by a primary containing either an ordinary spark or a quenched spark, said there was an apparent conflict between the usual theory and the facts disclosed by these curves. A pair of coupled circuits of equal frequency form in general a system of two degrees of freedom, which therefore possesses two periods of oscillation lying one on each side of the natural period of either circuit. The fact that Dr. Fleming's curves show, for some degrees of coupling, three humps instead of two, indicates that the pair of circuits does not remain a double system throughout the oscillation. One circuit disappears at a more or less early stage of the process, that is to say, the primary spark dies out and virtually removes the primary circuit from the combination, so that thereafter the secondary circuit vibrates alone. Usually it is assumed in the case of ordinary sparks between two balls that the spark endures almost to the end of the train of oscillations, and in the case of quenched sparks it is assumed that the primary spark is stopped at the end of one or two half swings. Dr. Fleming's curves show that both of these assumptions would be wrong in the case of his apparatus: the ordinary spark dies out sooner and the quenched spark later than is usually thought. The former fact is perhaps accounted for by the spark being placed in a blast of air. The curves show that in both cases the closeness of coupling has a profound effect on the length of life of the spark; with loose coupling the spark dies out much earlier than with close coupling.

Prof. G. W. O. **Howe** said he would like to ask Dr. Fleming what he meant exactly by "a dead-beat spark." Dr. Fleming used this expression several times in speaking of the impact discharger. It was stated in the Paper that "it is a dead-beat discharge," and "the spark is damped out of existence instantly," and again, "the discharge is practically non-oscillatory." He did not believe that this was an accurate discription of what actually took place in the primary circuit of a so-called impact discharger.

The primary and secondary circuits having been tuned to exact resonance and coupled together, the energy set oscillating in the former circuit by the breaking down of the spark-gap is gradually transferred to the secondary. The rate of this transfer depends on the tightness of the coupling, but with the tightest coupling used by Prof. Fleming, viz. 0·33, it would take a cycle and a half to transfer all the energy to the secondary circuit. If the spark was quenched under these conditions one might perhaps be justified in speaking of the transfer of energy as an impact. The resonance curves in Fig. 3 show that the spark is not quenched at this point, but that the energy surges backwards and forwards several times just as it does in an ordinary spark-gap in air. On loosening the coupling the transfer of energy takes place less rapidly, and the interval during which the primary current has a small value may be so long that the spark is quenched sooner. To enable the spark to be quenched at the first opportunity, that is, at the moment when the energy has been transferred to the secondary circuit for the first time, it appears from Fig. 1 to be necessary to reduce the coupling to 0·11. With this weak coupling, however, about 5 complete cycles are required to transfer the energy from the primary to the secondary circuit. If the oil-gap be replaced by the 1 mm. air-blast gap, Fig. 1 shows that the coupling has to be still further reduced to 0·045 before the spark is quenched at the first opportunity. The air-gap spark is more persistent than the oil-gap spark, but the difference between the two is merely one of degree.

The train of waves that follows each spark may be divided into two parts; during the first part we have beats between two coupled circuits, giving the two outside humps on the wave-meter curves; during the second part we have a single oscillatory circuit, giving the middle hump in the curves.

In view of the nature of the phenomena involved, he did not think we could speak of a dead-beat spark or discharge, but, under the best conditions as regards coupling, we may have a dead-beat surge of energy from the primary to the secondary circuit. For this reason he thought that the term "quenched spark" was preferable to "impact" or "shock excitation."

Mr. W. C. S. **Phillips** remarked that he had found Prof. Fleming's Paper on "The Energy Losses in Condensers" of great interest, because it seemed to have some bearing on the results of some comparative tests of the capacities of condensers which he made some time ago.

Each condenser was in turn made part of an oscillating circuit by putting it in series with a known constant inductance, and the frequency of the oscillations was measured by means of a Fleming cymometer.

From a knowledge of the inductance of the oscillating circuit and its oscillation constant, the capacity of each condenser was obtained. When using a parallel plate condenser immersed in oil, a neon tube was quite satisfactory for the determination of the oscillation constant. Without in any way altering the adjustment of the two circuits the parallel plate condenser was replaced by a leyden-jar of equal capacity. The cymometer current was now so weak that it was impossible to detect the position of resonance by means of the neon tube. As care was taken not to alter the coupling of the circuits, nor to introduce any resistance by bad contact into the oscillating circuit, he could only explain the discrepancy by supposing that there was a considerable loss of energy in the condenser. The current supplied to the induction coil used for the production of the spark discharge was, of course, kept constant.

Dr. **Fleming**, in reply, said he noticed that surprise had been expressed by several speakers that the dielectric losses are not more nearly proportional to the square of the voltage or electric force. With one or two exceptions, their experiments show a loss varying as some power between 2 and 3. He did not think that we could infer from experiments at low frequency what was likely to take place at high frequency, because there are some special sources of loss in the latter case. Thus, for instance, losses due to brush discharges from points and edges of the metal coatings are by no means negligible. It was found necessary to remove a brass indicating pointer from the variable oil condenser and replace it by an ebonite one, because a very obvious electric brush formed on the metal tip. These ionization losses might vary as some power greater than 2.

It is certain that these brush discharge losses are always present in the case of high-frequency measurements. The slight departure from the square law in the case of many of the results such as 2·15 instead of 2·0, may well be due to unavoidable errors of experiment. The method employed required care in the calibration and use of the high-frequency ammeter used. The objection raised by Mr. Campbell to the fact that we have to take the difference of the squares of two nearly equal currents is not so serious as it seems. The thermoelectric ammeter used is associated with a Paul single pivot galvanometer, and the scale of this last instrument is equidivisional, and the readings proportional to the mean-square value of the alternating current passing through the wires. Hence the square value was read at once. He was glad to find Mr. Jacob's interesting results so nearly in accord with his own, for ebonite condensers.

The observations made by Mr. Phillips on the difference between the effects of a leyden-jar and an oil condenser of the same capacity, when used with a cymometer and neon tube, were quite in accord with his own experience. They are undoubtedly a proof that there is a larger dissipation of energy in the case of the leyden-jar.

Mr. Rayner's careful analysis of our Paper does much to show that there is a high degree of probability that the principal part of the dielectric loss is, at any rate, proportional to the square of the electric force. When dealing with high-frequency oscillations there is, of course, much more difficulty in getting very accurate results than with low frequency. The observations we have taken would be impossible altogether without the use of an impact discharger, but it may be that the oscillations in the secondary circuit are not absolutely the free oscillations, and it is upon the assumption that they are free that our theory of the apparatus is based. As regards the last Paper on "Resonance Curves," Dr. Eccles puts the facts very concisely. There is undoubtedly a back effect produced, by the secondary oscillations, on the primary circuit, which results in a prolongation of the primary spark when close coupling is used, and this results in the appearance of the two-period oscillations superimposed on the free period oscillation in the secondary circuit.

Replying to Prof. Howe as to the sense in which the phrase "dead-beat spark" is used, Dr. Fleming said that it was intended to imply a spark which is very quickly quenched. The resonance curves given in their Paper prove that the quenching of the spark is not absolutely instantaneous, but that even with the so-called "impact discharger" the primary spark does endure for a very short time. If, however, the coupling does exceed a certain value, the result is to set up in the secondary only the free oscillations, and the advantages of the impact discharger are that this condition pertains for a closer coupling than is the case with the ordinary spark ball discharger. He agreed that the term "impact discharger" is perhaps not the best that could be used, but it is well understood, and as they have used it, it communicates to the secondary circuit just one non-reverberatory wave of energy which is then dissipated in the secondary circuit as heat.

XIII. *On Flames of Low Temperature Supported by Ozone.*

By the Hon. R. J. STRUTT, F.R.S., *Professor of Physics, Imperial College of Science, S. Kensington.*

RECEIVED JAN 16, 1911.

IN a Paper read before the Physical Society on Nov. 25, 1910, it was shown that the yellow afterglow often observed when an electric discharge has passed through a vacuum tube containing air, is due to the mutual reaction of nitric oxide and ozone, each formed in the discharge. The glow can, in fact, be imitated by allowing nitric oxide, chemically prepared, to mix with a stream of oxygen which has been through a vacuum tube at low pressure.

The intention was expressed of examining the action of ozone on other oxidisable gases, to see whether any analogous flame would be produced. The results of such experiments will now be described.

The apparatus employed has been modified to suit the conditions of particular cases, but the form shown in the figure may be regarded as typical. A vacuum discharge tube A is fed with a stream of oxygen through the stopcock B. The ozone thus produced issues through the jet C into the wider tube D, where it meets with a current of any gas under investigation, introduced through the stopcock E. The powerful air pump which is used to maintain the gas currents is attached to the side tube G. F is a quartz window, cemented on with sealing wax, through which the spectrum of the flame produced in the space D can be photographed, without losing the ultra-violet portion of the spectrum.

When the vapours of volatile liquids were to be investigated the apparatus was the same, with a bulb containing the liquid attached to E instead of the supply of gas. It is not advisable to allow such vapours to find their way into the pump, where they may dissolve in the oil used to lubricate it, and prevent the attainment of a good vacuum. To protect the pump, the gases were drawn through a tube loosely packed with copper oxide, on their way to it. This tube was heated to redness, and effectually oxidised any of the vapour which might be in excess of the ozone.

When it was desired to work with the vapour of a volatile solid, fragments were simply placed in the tube D, the window F being temporarily removed to admit them.

In photographing the spectra of the flames a little stray light from A was liable to find its way by reflection into D, and some of the brighter lines of the ozonising discharge were recorded, but these were readily eliminated by comparison with a direct photograph of the spectrum of this discharge. A small single prism quartz spectrograph by Hilger was employed, and good photographs of the spectra of the various flames were usually obtainable with an hour's exposure or less.

To observe the temperature of the various flames, a similar apparatus was employed, but with the tube D sufficiently prolonged to contain a mercury thermometer, which was placed so that its bulb was enveloped in the flame.

The phenomena obtained in individual cases will now be described.

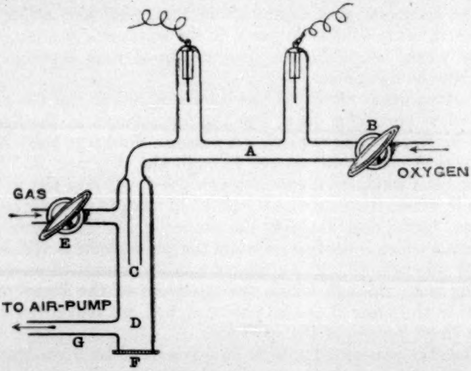

FIG. 1.

Sulphuretted Hydrogen.—A stream of this gas mixed with ozone gave a sky-blue flame free from the " muggy " appearance of the nitric oxide flame. The temperature recorded was 70°C. This flame, in common with all those to be described, was much inferior in luminosity and volume to that given by nitric oxide. The chemical action takes place in a restricted region, and a sensible rise of temperature is obtained. 70°C. was reached, but this of course depends on the exact conditions of pressure.

The spectrum ranged from $\lambda 2,300$ to $\lambda 4,800$. Over most of this range it was continuous, but in the extreme ultra-violet, from $\lambda 2,300$ to about $\lambda 2,600$ bands could be distinguished, and it is probable that they extend further, though masked by the continuous spectrum.

Sulphur.—Instead of feeding a stream of sulphuretted hydrogen into the stream of ozone, a fragment of solid sulphur was placed in its path, at D (Fig. 1). A few seconds after the ozone current was started a blue flame, indistinguishable from the former one, was seen to play round the sulphur. As the experiment continued, the sulphur became warm, and melted, showing visible signs of ebullition, of course, at the low gaseous pressures at which the experiment was conducted.

The slight interval of " hang fire " before the glow commences no doubt represents the time required for the sulphur to get hot enough to give an appreciable amount of vapour. If the electric current was turned off, the glow at once ceased in this case as in all the others. The combustion only occurs at these low temperatures and pressures in ozone. It is not maintained when oxygen is substituted.

The spectrum of the sulphur glow was photographed, and was indistinguishable from that of the sulphuretted hydrogen glow. Mr. Eagle kindly photographed the spectrum of sulphur burning in air. It is roughly co-extensive with the former spectrum, but does not show the same system of bands. With atmospheric combustion, bands occur at the visible end of the spectrum.

It appears that sulphur burned in either way gives the same continuous spectrum ; superposed on this is a system of bands different in the two cases. Further study of these bands must be left to specialists in spectroscopy.

Carbon Disulphide gave a blue flame, apparently the same as sulphur or sulphuretted hydrogen. The spectrum was not examined.

There can, I think, be no doubt that the blue flame observed in these experiments is the same as the blue after-glow observed by Newall * with the electrodeless discharge, which he connected with oxygen and sulphur. The connection is, in fact, the simplest possible, the glow being produced by combustion of sulphur in ozone.

Sulphur Dioxide gave no glow with ozone.

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* " Proc." Camb. Phil. Soc., Vol. IX., p. 295, 1897.

Selenium in the ozone stream gave no glow in the cold; but when gently heated it was surrounded by a not very conspicuous glow. This was not examined further.

Arsenic gave no glow and was not visibly acted on.

Iodine gives an orange glow, redder than that due to nitric oxide. It is oxidised to a yellowish white solid, presumably iodine pentoxide.

The glow is fairly bright to the eye, but no spectrum was recorded on the plate by a 20 minutes' exposure. Visual observations showed that the spectrum was confined to the red and yellow regions, which accounts for the failure to photograph it. So far as could be observed, the spectrum was continuous.

Hydrogen, Ammonia, Carbon Monoxide gave no glow. *Nitrogen* and *nitrous oxide* were shown in the former Paper (*loc. cit.*) to give none.

Acetylene, in contrast to most hydrocarbons gives a fairly conspicuous glow, of bluish green colour. A thermometer immersed in this glow indicated 100°C. under the particular conditions of the experiment. The spectrum consisted of the Swan bands, together with the hydrocarbon band at λ4,315, and is thus identical with that of the inner cone of a Bunsen flame. The colour of the flame can be regulated by the acetylene supply. With a small supply it is green, the Swan bands being predominant. With an increased supply the flame contracts, the ozone being now consumed before it has got far from the jet where it issues into the acetylene. At the same time the colour becomes bluer, and the hydrocarbon band brighter, relatively to the Swan bands, than in a Bunsen flame. The ultra-violet bands of water vapour are not obtained from the acetylene glow.

The chemical action occurring in this glow has as yet only been partially studied. The gases from the glow were drawn through a U-tube cooled in liquid air on their way to the pump, with a view to retaining condensible products. When the glow had been maintained for some time this U-tube was detached, and the gases condensed in it collected through a Toepler pump, for analysis. The following experiment was typical:—

Acetylene admitted into the apparatus	17·3 c.c.
Total gas collected in U-tube	11·0 c.c.
After absorption with caustic potash	8·0 c.c.

The remainder of the gas was absorbed almost completely by fuming sulphuric acid, and presumably consisted of unburnt acetylene. Thus the acetylene actually burnt amounted to 17·3-8, or 9·3 c.c. This only yielded 3 c.c. of carbon dioxide, whereas its complete oxidation would yield 18·6 c.c. It is clear, then, that the oxidation stops at some intermediate stage. Carbon monoxide is probably the chief product, and would, of course, escape condensation by liquid air.

Benzene under some conditions gave a just perceptible glow, too faint to be easily studied, though attended by a notable rise of temperature. On one occasion, when the glow was not observable, a white solid deposited on the glass. This exploded spontaneously with a loud report, breaking the apparatus, and leaving a deposit of carbon. It was probably the "ozobenzene" of Houzeau and Renard.*

Methane, *Ethylene* and the vapours of *Petrol*, *Ether* and *Alcohol* gave no glow.

Napthalene and *Camphor* also failed to give a glow, though energetically acted on. The former was carbonised by ozone. The latter was changed to a dull opaque white solid, easily distinguished from the glittering translucent surface of the original camphor.

Cyanogen gave a white glow, with a purple tinge near the nozzle, when the ozone issued into it. The spectrum shows the cyanogen bands at λ388 and λ387 very strongly, also those at λ359 and λ421.

In addition to these, the spectrum shows a band at λ431, which is possibly the hydrocarbon band 431·5, introduced in consequence of the gas having been collected over water; and another unidentified band at λ415, which may eventually prove of interest in connection with the spectra of comets.

In this case, as in all the spectroscopic part of the work, I am much indebted to the kind assistance of my colleagues, Prof. Fowler and Mr. Eagle. Prof. Fowler may perhaps pursue the work on the cyanogen spectrum.

Further experiments on other aspects of the subject are in progress.

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* "Comptes Rendus," 76, p. 572, 1873.

XIV. *The Caloric Theory of Heat and Carnot's Principle.** *By* H. L. Callendar, *M.A., LL.D., F.R.S., Professor of Physics at the Imperial College of Science and Technology, S.W.*

"Difficile est proprie communia dicere."

1. *Introductory.*—It is the time-honoured privilege of a President on these occasions, in place of providing new and original material for discussion, to indulge in the retrospect or review of some branch of physical science in which he may be particularly interested. I propose to take advantage of this presidential privilege by submitting for your approval some reflections on the foundations of the science of heat. So much has been written, and still continues to be written, on this engrossing subject from the purely theoretical standpoint, that I should not venture to abuse your patience by choosing so hackneyed a theme if I did not feel that, by spending the best part of my life in the practical teaching and experimental investigation of the science, I had earned the right to an expression of opinion. I feel sure that, however, you may disagree with my way of putting things, you will at least believe that it is the outcome of practical experience, and represents the point of view of a serious experimentalist. The subject chosen is so large and the time at my disposal so limited that I must be content with the merest outline of a suggestion, and must leave you to fill in the details yourselves. It is impossible to cover the whole ground, or to refer to a tithe of the ideas or theories which have been from time to time proposed for developing the fundamental principles of the science. If I should appear to have omitted many considerations of importance I trust you will give me the benefit of the doubt, and believe that the omission is due merely to exigencies of space or time over which even a physicist has but a limited control.

The caloric theory of heat is now so long forgotten that we rarely hear it mentioned, except as an example of primeval ignorance; but it was not really quite so illogical as it is generally represented to be. The science of heat might have been developed along the lines of the caloric theory nearly, if not quite, as well as on the lines of the dynamic theory. Some most important and fundamental ideas, which are obscured in the early stages of the present method of exposition, would, in fact, have been brought into greater prominence and more widely appreciated, and a clearer view of the whole subject might thus have been obtained.

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* Presidential Address, February 10, 1911.

The original conception of caloric as a subtle imponderable fluid, capable of flowing from one body to another, of expanding bodies, and of producing rise of temperature or change of state, was, in the main, merely a picturesque analogy, invented, like the electric or magnetic fluids, with the object of giving the mind a tangible something to reason about, a measurable entity remaining constant in various transformations or exchanges. The fluid terminology remains a convenient method of expression even if the thing measured has not all the properties of a material fluid.

The first serious difficulties which the theory encountered were in explaining the apparent generation of heat by friction or compression. It might have been boldly assumed, as Cavendish and others maintained, that heat was generated by friction in the same kind of way as electricity; but a more generally acceptable explanation at the time appeared to be that some of the latent heat was ground or squeezed out of the bodies concerned and became sensible. The abraded or compressed matter, according to this view, was not capable of containing so much heat as the original stuff, or its " capacity for heat " was diminished. As there were no means of measuring the total heat content of a body, the " thermal capacity " was generally regarded as being proportional to the quantity of heat required to raise the temperature one degree, in which sense the phrase is still employed. The possibility of this explanation of the production of heat by friction was negatived at an early date by the experiments of Rumford and Davy. Rumford (" Phil. Trans.," 1798) in one of his experiments using a boring machine with a blunt tool, succeeded in raising 26·5 pounds of cold water to the boiling point by means of friction in 2½ hours with the production of only 4,145 grains of metallic powder. He then showed that the metallic powder required the same quantity of heat to raise its temperature 1 deg. as an equal weight of the original metal, or that its " capacity for heat " in this sense was unaltered. He argued that so small a quantity of powder could not possibly account for all the heat generated, and that the supply of heat appeared to be inexhaustible. Heat could not, therefore, be a material substance, but must be something of the nature of motion, as Newton and Bacon had surmised. The argument was not quite conclusive, because he was unable to show that the powder really contained the same total amount of heat as the solid metal. But the experiments must have satisfied every reasonable philosopher of the extreme improbability of the explanation then generally accepted. Davy's experiment (described in an essay "On Heat, Light and Combinations of Light," 1799), in which he melted two blocks of ice by rubbing them together, was more conclusive, since as he remarks "it is a well-known fact that the capacity of water for heat is greater than that of ice, and ice must have an absolute quantity of heat imparted to it before it can be converted into water. Friction consequently does not diminish the capacities of bodies for heat." In stigmatising the "last eleven words" as "a lame and impotent conclusion," Lord Kelvin (Ency. Brit., "Heat," 1882) inadvertently does Davy an injustice, because Davy evidently uses the phrase "capacity for heat" in both senses, as was common at the time. In a later passage of the same essay he defines capacity for heat as the total thermal content of the body, and employs the unfamiliar phrase "capability of temperature" to denote the heat required to raise the temperature. 1 deg.

Although it was clear from these and similar experiments that caloric was actually generated by friction and could not be regarded as a material fluid in the ordinary sense, insomuch as it did not in all cases obey the law of the conservation of matter, then recently established, the idea of heat as "a mode of motion" was at that time too vague to afford a satisfactory substitute for the fluid theory. Davy speaks of heat as "repulsive motion," and distinguishes it from light, which is "projective motion," and is capable of forming compounds with ordinary matter. Thus oxygen gas is not a simple substance, but a compound, termed by Davy "phosoxygen," of light and oxygen. A theory of this kind could not be usefully applied until it was shown how the motion corresponding to heat should be measured (whether by its momentum or *vis viva*, or otherwise), how much motion was required to generate a given quantity of heat, and whether the ratio of equivalence between heat and motion suitably measured was in all cases invariable. These questions were, in fact, so difficult to settle that we can hardly wonder that it was 50 years before a satisfactory answer was obtained.

2. *Thermal Properties of Gases.*

Light was first thrown on the subject by investigations bearing on the thermal properties of gases. The laws of expansion of gases and the elementary properties of vapours were already familiar about this time from the researches of Dalton, Gay Lussac and others; but the data regarding the specific heats of gases were very meagre and conflicting. Dalton had found a rise of 50°F. produced by compressing air suddenly to half its volume. This was less than a third of the true rise of temperature, but in any case the result could not be interpreted in terms of quantity of heat generated without a knowledge of the specific heat.

The first reliable results for the specific heats were obtained shortly afterwards by Delaroche and Bérard (" Ann. Chim.," 1813). Their constant pressure method was a great advance on all previous work, and gave the mean specific heats referred to water between 15°C. and 100°C. They found that equal volumes of the permanent gases H_2, N_2, O_2 and CO at atmospheric pressure had nearly the same thermal capacities as air, the specific heat of which they found to be 0·267 calorie per gram at a pressure of 740 mm. of mercury. The compound condensable gases, CO_2, N_2O and C_2H_4, were correctly found to have larger thermal capacities per unit volume than air. They were unable to state whether the specific heats increased or diminished with temperature, but from an experiment with air at 1,000 mm. pressure, which gave the value 0·245 calorie per gramme, they concluded that the specific heats diminished with diminution of volume. The experiments of Regnault 40 years later showed that this observation was incorrect, but it appeared at the time to lend support to the view that the rise of temperature observed on suddenly compressing a gas was due to the diminution of its specific heat, a hypothesis otherwise unsupported by experimental evidence.

It was evident that the elasticity, or resistance of a gas to compression, would be greater in a rapid or adiabatic compression, on account of the rise of temperature, than in a slow compression at constant temperature. Laplace was the first to see in this fact the probable explanation of the long-standing discrepancy between the observed velocity of sound and that calculated by Newton on the basis of Boyle's law of isothermal compression. According to Boyle's law, a reduction of 1 per cent. in volume at constant temperature raised the pressure of air by 1 per cent. The observed value of the velocity of sound required that in a rapid compression of 1 per cent. the pressure should be increased 1·41 per cent. This result is more accurately expressed by the statement that the ratio of the adiabatic to the isothermal elasticity of air is 1·41. At a later date (" Ann. Chim.," 1816) Laplace stated that he had succeeded in proving that the ratio of the adiabatic to the isothermal elasticity of a substance was the same as the ratio of the specific heat at constant pressure to the specific heat at constant volume. This important result follows directly from the definitions of the two elasticities and the two specific heats, and is independent of any view as to the nature of heat or temperature. But it appeared at the time to afford the strongest support to the caloric theory, because it showed that the specific heat was actually greater when the volume was allowed to increase than when the volume was kept constant, and because no explanation was forthcoming on the kinetic theory. The specific heats measured by Delaroche and Bérard were those at constant atmospheric pressure. All attempts to measure the specific heats at constant volume had signally failed on account of experimental difficulties, but their values could be calculated from the known value of the ratio thus deduced by Laplace.

A few years later Clément and Désormes (" Journ. de Phys.," 1819) succeeded in verifying the value of the ratio deduced from the velocity of sound by directly measuring the changes of pressure produced by adiabatic and isothermal compression. They found the value 1·354, which was lower than the true value 1·41, because the compression in their experiments was not strictly adiabatic. Gay Lussac and Welter (" Ann. Chim.," 1822) repeated the experiment with slight improvements, employing expansion instead of compression, and found a rather better approximation, namely, 1·375. They found the ratio practically constant for a range of pressure from 144 to 1,460 mm., and for a range of temperature from —20°C. to +40°C. Assuming on this evidence the constancy of the ratio of the specific heats of air, Laplace (" Œuvres," V., p. 143) showed that the specific heat per unit volume at a pressure *p* should vary as $p^{1/\gamma}$, and the specific heat per unit mass as $p^{1/\gamma-1}$, according to the caloric theory, where γ is the ratio of the specific heats. This result happened to agree precisely with the values found by Delaroche and Bérard for air at 740 and 1,000 mm., and appeared to be an additional confirmation of the caloric theory. The same propositions were demonstrated by Poisson (" Ann. Chim.," 1823, XXIII., p. 337), who also gave the familiar relation, $pv^{\gamma}=$constant, between the pressure and volume of a gas in adiabatic expansion or compression. The last equation is evidently correct, but the reasoning with regard to the variation of the specific heat with pressure is somewhat obscure, and the result given does not appear to follow necessarily from the caloric theory.

3. *Carnot's Reflexions on the Motive Power of Heat.*

The greatest step in advance was made about this time by Carnot in his famous essay, entitled " Reflexions on the Motive Power of Heat," Paris, 1824. Proposals had frequently been made to employ other agents, such as air, alcohol or ether, in place of steam in a heat engine, with a view to securing higher efficiency or a greater quantity of motive power for a given consumption of fuel. Carnot proposed to find the answer to the questions, how the efficiency was limited, and whether other agents were preferable to steam. In considering this problem he first points out that motive power (which he measures in kilogrammetres, and employs as equivalent to the modern term " work ") cannot be said to be produced from heat alone, unless nothing but heat, or its equivalent fuel, is supplied. All parts of the engine, including the working substance or agent, must be at the end of the process in precisely the same state as at the beginning. He here assumes a fundamental axiom, which had always been taken for granted, but not so precisely and explicitly stated by previous writers.

Carnot's Axiom for a Cyclical Process.

" When a body has undergone any changes and after a certain number of transformations is brought back identically to its original state, considered relatively to density, temperature and mode of aggregation, it must contain the same quantity of heat as it contained originally."* The ideal engine for theoretical purposes must, therefore, be supposed to work in a complete cycle of operations in which everything is restored to its initial state, except that a certain quantity of heat has been supplied to and utilised by the engine. This does not seriously limit the application of the theory, because, although practical engines do not always use the same identical mass of working substance for each stroke, they necessarily repeat a regular cycle of operations continuously, and it is generally possible to devise an equivalent theoretical cycle in which the working substance is restored after each stroke to its initial state. Carnot proceeds to show that motive power cannot be obtained from heat by alternate expansion and contraction of the working substance, without postulating the existence of two bodies at different temperatures to serve as boiler and condenser, or source and sink of heat respectively; and conversely that, wherever a difference of temperature exists, it is possible to utilise it for the production of motive power. He thus deduces the fundamental rule for obtaining the best results.

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* Carnot goes on to say: "In other words, the quantities of heat absorbed or disengaged in its diverse transformations are exactly compensated. This fact has never been doubted; it has been first assumed without reflection, and then verified by calorimetric experiments. To deny it would be to upset the whole theory of heat, for which it serves as a basis. For the rest, one may say in passing, the principal foundations on which the theory of heat rests, require the most attentive examination. Many experimental facts appear almost inexplicable in the present state of this theory." Clausius "Pogg. Ann.," 1850; "Phil. Mag.," IV., vol. II., p. 2, 1851) misquotes this passage most ingeniously. He fails to notice that the essential point assumed by Carnot in any cyclical process is that the quantity of heat remaining in the body is the same when it has been brought back identically to the same state. He makes Carnot "expressly state that no heat is lost in the process, that the quantity (transmitted from the fireplace to the condenser) remains unchanged." Clausius makes *this* "the fact which has never been doubted," &c. In reality, Carnot, in describing his cycle, does not make the assumption which Clausius here attributes to him, because it is not a necessary part of the reasoning. Carnot does not, even in the paraphrase of his axiom, say that the quantities of heat absorbed or disengaged in its diverse transformations are *equal*, but merely that they are *exactly compensated*, so far as the body itself is concerned. Whether they are equal or not depends on the way in which a quantity of heat is measured. The essential point of the reasoning in Carnot's description of his cycle is that the result is independent of any assumptions with regard to the way in which temperature or heat are measured, provided that the quantity of heat remaining in the body is the same when it has been brought back to its original state after any transformations. The special word "compensated," which Carnot employs in place of "equal" in paraphrasing his axiom, would appear to imply that he did not consider the quantities of heat absorbed and disengaged necessarily equal, and that this was one of the points which "required the most attentive examination."

Carnot's Criterion of Thermal Reversibility.

" In order to realise the maximum effect, it is necessary that, in the process employed, there should not be any direct interchange of heat between bodies at sensibly different temperatures." Direct transference of heat between bodies at different temperatures would be equivalent to wasting a difference of temperature which might have been utilised for the production of motive power. This rule excludes all the well-recognised thermal sources of waste of power, which practical engineers had already been most successful in reducing. The ideal engine was also assumed to be free from mechanical losses such as friction. Under these conditions there is equilibrium, both mechanical and thermal, at every stage of the process, so that each operation is reversible, and may be supposed to be performed in either direction at will.

4. *Carnot's Ideal Cycle.*

Carnot first gives a rough illustration of an imperfect cycle, using steam in much the same way as in an ordinary steam engine, but supposing most of the usual losses absent. After expansion to condenser pressure, the steam is supposed to be completely condensed, and then returned as cold water to the hot boiler. He points out that the last stage does not conform to his rule for maximum effect, because, although the water is restored to its initial state, there is direct passage of heat from a hot body to a cold body in the last process. Having thus established the general notions of a perfect cycle, and of reversibility as the criterion of perfection, he proceeds to give a more exact illustration, employing a gas as the working substance. In this demonstration he assumes only the well-established experimental facts (1) that a gas may be heated or cooled by rapid compression or expansion, and (2) that, if compressed or expanded slowly in contact with conducting bodies, it may be made to evolve or absorb heat at constant temperature. Carnot's cycle itself is so familiar that it is scarcely necessary to describe it in detail. It is much to be regretted, however, that it is not always given as nearly as possible in Carnot's own words. His description is so nearly perfect that it could hardly be improved. Details which may appear superfluous at a first perusal are seen, on more careful study, to be exactly in the right place. Unnecessary assumptions have been introduced by others in describing the cycle, and have led to mistakes which Carnot carefully avoided. For instance, Clapeyron, in describing the cycle, directs one to compress the gas at the lower temperature in contact with the cold body B until the heat disengaged is equal to that which has been absorbed at the higher temperature. This operation would be very difficult to perform, and involves the unnecessary assumption that the gas at this point contains the same quantity of heat as it contained at the beginning of the first stage, so that when the body B is removed, the gas will be restored to its original temperature by adiabatic compression to its original volume. It was chiefly for this reason that Clausius, who was acquainted with Carnot's work chiefly through the medium of Clapeyron's summary, so completely misinterpreted it, and substituted another proof. The same mistake undoubtedly induced Lord Kelvin to state (" Phil. Mag.," IV., 1852) that " Carnot's original demonstration utterly fails," and was the cause of the " corrections " attributed to James Thomson and Clerk Maxwell respectively. In reality, Carnot's original description is independent of any assumption as to the nature of heat, and requires no correction.

After completing the description of the cycle, and showing its exact reversibility, Carnot proceeds to say : " The impossibility of producing by the agency of heat alone a quantity of motive power greater than that which we have obtained in our first series of operations, is now easy to prove. It is demonstrated by reasoning exactly similar to that which we have already given. The reasoning will have in this case a greater degree of exactitude ; the air of which we made use to develop the motive power is brought back at the end of each cycle of operations precisely to its initial state, whereas this was not quite exactly the case for the vapour of water, as we have already remarked." Carnot considered the proof too obvious (as indeed it is) to be worth repeating. Unfortunately his original demonstration, referring to an imperfect cycle given merely *comme un aperçu*, as an introduction to the method, is not so exactly worded that exception cannot be taken to it. I will, therefore, repeat the proof in a slightly more definite and exact form, as Carnot probably intended it to run. " If it were possible to produce from a given quantity of heat supplied a greater quantity of motive power than that obtained from a reversible engine, it would suffice to divert a portion of this power to return to the source by means of a reversible engine the quantity of heat taken from it. We should thus obtain at each repetition of the cycle a balance of motive power without taking any heat from the source—that is to say, without any consumption of fuel." The extreme improbability of such a result is a sufficient *reductio ad absurdum* to satisfy any reasonable intelligence. Whether the condenser is heated or cooled in the process is immaterial for practical purposes. The condenser might be the ocean or the whole earth. We should thus obtain the practical equivalent of a perpetual motion, interpreted by Carnot as the continuous creation of motive power. If the conservation of energy is assumed, it is true that the condenser would be cooled. But it hardly seems necessary to invoke the law of the conservation of energy, and to make a final appeal to axioms, such as those of Kelvin* or Clausius, which are far less self-evident. The conservation of energy as applied to heat is itself an experimental law, and the final appeal must be to experiment in any case.

5. *Carnot's Principle.*

Carnot stated his principle as follows:—

"The motive power of heat is independent of the agents set at work to realise it; its quantity is fixed solely by the temperatures of the bodies between which in the limit the transfer of heat is effected."

He adds to this statement: "It is necessary to understand here that each of the methods of developing motive power attains the perfection of which it is susceptible. This condition will be fulfilled if, as we have remarked above, there is not produced in the body any change of temperature which is not due to a change of volume, or, what is the same thing otherwise expressed, if there is never contact between bodies at sensibly different temperatures." He also explains that the principle applies equally to different substances and to different states of the same substance, provided that the temperature limits are the same for the agents compared in any case.

Carnot's principle may be stated in a great variety of different ways, which are more or less equivalent, but it seems best to adhere as closely as possible to Carnot's own words, the meaning of which is sufficiently clear and exact for all practical purposes. Whatever form of words is adopted, it will always be necessary to define the terms employed, and to explain how the principle is to be applied. On this understanding many different modes of expression are equally valid and satisfactory but many paraphrases of Carnot's principle have been given from time to time which appear to be distinctly objectionable as obscuring the real points at issue. Clausius, for instance (" Pogg. Ann.," 1850, Vol. LXXIX., p. 369) quotes Carnot's principle as follows: " The performance of work is equivalent to a transference of heat from a hot body to a cold body without the quantity of heat being thereby diminished." This method of statement omits both the essential points, (1) that the efficiency is independent of the working substance, and (2) that it is a function solely of the temperature limits. It also introduces an unnecessary assumption with regard to the manner in which a quantity of heat is measured, which is not an essential part of Carnot's reasoning. The principle itself is independent of the manner in which either heat or temperature is measured. Provided that the methods of measuring heat and temperature are consistent, and are the same for all substances compared, the only effect of altering the temperature scale or the definition of the heat unit, is to alter the form of the function representing the efficiency.

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* Carnot's proof does not assume either that heat is measured as energy or that energy is conserved. If both these points are assumed, his axiom that it is impossible to make a heat engine work without taking heat from the source, or without consumption of fuel, is equivalent to Kelvin's axiom that motive power cannot be obtained by cooling the condenser. One may be deduced from the other; but Carnot's axiom is the most direct result of experience with heat engines, involves the fewest assumptions, and appears to be the most appropriate for the required formal proof.

6. *Carnot's Applications of his Principle.*

Carnot endeavoured to apply his principle to the greatest possible variety of cases, and to subject it to the most complete experimental verification, so far as the data available at that time would permit. He succeeded in establishing for the first time several important results, which have in some cases been overlooked or attributed to others. He appears to have worked out most of these results analytically in the first instance as indicated by his footnotes, and to have subsequently translated the formulæ into words in the text for the benefit of his non-mathematical readers. He expressed his principle analytically in the following simple form. If W is the quantity of work or motive power obtained in a reversible cycle from a quantity of heat, Q,* communicated to the working substance at a temperature, t, C, the condenser, being assumed for convenience at 0°C., we may write

$$W/Q=F(t), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

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* It is not here assumed that heat is necessarily measured as energy.

where $F(t)$ is some function of the temperature limits, depending on the way in which temperature and heat are measured but independent of the properties of the working substance. Owing to the limited state of knowledge with regard to the physical properties of bodies at that time, he was unable to apply the principle generally in this form. He points out that a great simplification is introduced in the application of the principle by considering a cycle of infinitesimal range, dt, but finite heat absorption Q, at a temperature t. He thus obtains by differentiation the corresponding expression,

$$dW/dt = QF'(t), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $F'(t)$ is the derived function of $F(t)$, and is generally known as Carnot's function. This function evidently denotes the quantity of work obtainable per degree fall in a Carnot cycle per unit of heat supplied at the temperature t; and is the same for all substances at the same temperature.

Carnot, in the first instance, by applying the principle in this form to the discussion of the thermal properties of gases, endeavoured to complete the solution of the problem from the theoretical side by showing how the value of his function $F(t)$ or $F'(t)$ might be expected to vary with temperature, consistently with the ascertained laws of gases, and with various simple assumptions made with regard to properties which had not been experimentally determined.

7. *Properties of Gases deduced by Carnot from his Principle.*

Carnot's discussion will be followed as closely as possible with a few slight changes in the notation to render it more familiar. The work done in isothermal expansion at a temperature, T (Carnot writes $t+267$) by a gas obeying the law $pv=RT$, in expanding from v_o to v is $RT \log_e (v/v_o)$. The work done in a cycle of range dT per 1 deg. fall is evidently $dW/dT = R \log_e(v/v_o)$. By Carnot's principle this is equal to $QF'(t)$, where Q is the corresponding quantity of heat absorbed. We thus obtain the expression for the heat absorbed by a gas in isothermal expansion,

$$Q = R \log_e (v/v_o)/F'(t), \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which must be the same for all gases at the same temperature for the same ratio of expansion v/v_o, provided that they obey the law $pv=RT$, and that corresponding quantities of the different gases are taken (equal volumes at the same standard temperature and pressure) so that the constant R is the same for all. In other words, the heat absorbed by a gas in isothermal expansion is proportional to the whole work done, and bears to it the ratio $1/\mathrm{TF}'(t)$. This result is independent of the way in which temperature and heat are measured, provided that the gases obey the same law of isothermal expansion and give the same scale of temperature.

Since, when a gas absorbs a quantity of heat equal to the difference of the specific heats at constant pressure and volume, the work done in isothermal expansion is pv/T or R, Carnot deduces from the above general result that the difference of the specific heats (for equal volumes measured under the same conditions) must be the same—namely, $\mathrm{R}/\mathrm{TF}'(t)$—for different gases at the same temperature, and must also be independent of the pressure. He draws up a table showing the values of the specific heats at constant volume deduced from the results of Delaroche and Bérard at constant pressure. On this he remarks that the *ratio* of the specific heats must be different for different gases, whereas it had generally been assumed to be the same. The elevation of temperature produced by a sudden compression must therefore be different for different gases, and he gives a table showing the results.

Since, according to the experiments of Gay Lussac and Welter the ratio of the specific heats of air is independent of the pressure and temperature, and according to his own theory the difference of the specific heats must also be independent of the pressure at the same temperature, it follows that the specific heats themselves must be independent of the pressure. Carnot points out that this is inconsistent with the result of Delaroche and Bérard previously cited for the specific heat of air at 1,000 mm. pressure—namely, 0·245—which is less than the value 0·267 found at 740 mm. The discrepancy must be due to errors of experiment. The variation of the specific heats with pressure was doubtful, because it rested on a single difficult experiment, and the range of pressure was too restricted for a satisfactory conclusion to be drawn.

8. *Carnot's Application of the Caloric Theory.*

The results so far obtained by Carnot in his investigations with regard to the specific heats of gases, were independent of the caloric theory and remain equally true on the kinetic theory, although they have frequently been credited to other writers.* Because in some cases he employed the language and reasoning of the caloric theory, it does not follow, as most writers have hastily assumed, that his whole argument breaks down. In attempting to unravel the relations of the specific heats, Carnot was compelled to employ some theory or assumption as to the nature of heat. He naturally adopted the caloric theory, because no other theory was available at that time, and the experimental data were too inadequate to permit of any other method of treatment.

The most fundamental postulate peculiar to the caloric theory is that the quantity of caloric required to be added to a body to transform it from any state, defined, we may suppose, by temperature t_0 and specific volume v_0, to any other given state, defined by temperature t and specific volume v, is the same, in what ever way the change is effected. Carnot applies this assumption to the problem in hand in the following manner. Suppose that unit mass of gas is first heated at constant volume v_0 up to the temperature t, absorbing a quantity of caloric U_0, and is then expanded at constant temperature t, absorbing a quantity of caloric $R \log_e (v/v_0)/F'(t)$, as proved in the last section. Next suppose that the gas is first expanded from v_0 to v at constant temperature t_0, absorbing a quantity of caloric given by $R \log_e(v/v_o)/F'(t_o)$, and is then heated at constant volume v from t_o to t, absorbing a quantity of caloric U. The total quantity of caloric Q required in either case must be the same on the caloric theory. Whence we have,

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* Dulong ("Ann. de Chim.," XLI., p. 156, 1829), adopting a method due to Chladni, compared the velocities of sound in different gases by observing the pitch of a note given by the same tube when filled with the gases in question. He thus obtained the ratios of the specific heats. For O_2, H_2 and CO the ratios were the same as for air. But for CO_2, N_2O and C_2H_4 the ratios were much smaller than for air. On comparing his results with the specific heats for the same gases found by Delaroche and Bérard, Dulong observed that the changes of temperature for the same compression were in the inverse ratio of the specific heats at constant volume. He thus deduced the important conclusion, agreeing with that obtained theoretically by Carnot five years earlier, that "equal volumes of all gases under the same conditions evolve on compression the same absolute quantity of heat." Carnot's result is more general and significant, and it is clear that he deserves some credit for the prediction.

Joule in 1845 showed by experiment that the ratio (Carnot's $TF'(t)$) of the work done to the heat evolved in compressing a gas was nearly, if not quite, the same as the ratio J of work spent in heating water by friction to the heat evolved. The mechanical equivalent J must, therefore, be equal to Carnot's $TF'(t)$, if heat is measured as energy. Since Joule's experiments were all performed practically at one temperature, they do not afford direct evidence as to the variation of $F'(t)$ with temperature, unless we assume that a quantity of heat is measured as a quantity of energy which cannot vary with temperature. Admitting this, $F'(t)$ must be equal to J/T, as Joule himself suggested in a letter to Kelvin in 1848. Carnot is entitled to some credit for having predicted that the ratio must be constant at any one temperature, or must be a function of the temperature only. Mayer in 1842 assumed the equivalence on the basis of the old principle "*causa æquat effectum*," which could hardly be regarded as scientific demonstration.

$$U_o + R \log_e (v/v_o)/F'(t) = Q = U + R \log_e (v/v_o)/F'(t_o). \quad . \quad (4)$$

From this equation it is immediately obvious that, if the specific capacity for caloric at constant volume is independent of the volume, since $U = U_o$, $F'(t)$ must be equal to $F'(t_o)$; or Carnot's function must be independent of the temperature, if the latter is measured on the gas scale and if the heat is measured as caloric.* We also observe, as Carnot points out, that it is not necessary for the specific capacity for caloric to be a function of the volume in order to explain the rise of temperature produced by compressing a gas. But this assumption had so frequently been made by Laplace and others that it had come to be regarded as an essential part of the explanation of the phenomenon on the caloric theory. Carnot also investigates the general law of the variation of specific heat with pressure, admitting the result of Delaroche and Bérard as correct. But since we know now, from the observations of Regnault, first published in 1852, that the result of Delaroche and Bérard was wrong, and that the specific heat of air is practically independent of the pressure, it is clear that Carnot deserves some credit for having given the correct solution of the problem on this assumption.

Putting $F'(t) = \text{constant} = A$, Carnot obtains for the work done per unit of caloric supplied at a temperature t°C. in a finite cycle of range t to 0°C., the simple expression $F(t)=At$. Expressing the same result in terms of temperature T on the gas scale, we obtain for the work, W, due to a quantity, Q, of caloric supplied at a temperature, T, in a Carnot cycle of range T to T_o, the expression

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* The corresponding expression for $F'(t)$ on the mechanical theory may be obtained by an exactly similar process. If a quantity of heat is measured as energy, the difference of the quantities of heat absorbed and rejected in the cycle represented by equation (4)—namely, $U_o - U + R \log_e (v/v_o)[1/F'(t) - 1/F'(t_o)]$—must be equal to the external work done in the cycle—namely, $R \log_e (v/v_o)(T - T_o)$—divided by the mechanical equivalent J. If the specific heat at constant volume is independent of the pressure $U = U_o$, and $F'(t)$ must be equal to J/T. Substituting this value of $F'(t)$ in Carnot's expression (3) for the heat absorbed by a gas in isothermal expansion, we see that it is equivalent to the work done provided the gas obeys Boyle's law, and that its specific heat at constant volume is independent of the pressure. Clausius at a later date reversed this procedure. He began by assuming the equivalence of the heat absorbed to the work done by a gas in isothermal expansion, and deduced that the specific heat was independent of the pressure. But Carnot's method appears more logical and direct. The assumption of the equivalence of heat and work in a non-cyclical process, thus made by Mayer and Clausius, was, in effect,a violation of Carnot's fundamental axiom, and could not be justified theoretically. Carnot's assumption is also preferable from the experimental standpoint, because it proposes a more direct and simple test—namely, that the specific heat should be the same at different pressures.

$$W=AQ\,(T-T_o). \qquad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This expression is less general than equation (1), because it assumes that a quantity of heat is measured as caloric, and that temperature is measured on the scale of a gas obeying Boyle's law and possessing the property that its specific heat at constant volume is independent of the pressure. Admitting these assumptions the solution is obviously correct. The quantity of caloric, Q, supplied at the temperature, T, is necessarily the same as the quantity rejected at the temperature T_o, because the quantity of caloric in the working substance must remain constant, when no heat is supplied, in reversible expansion or compression such as is postulated by Carnot in his cycle. We observe that, if heat is measured as caloric, it is quite correct to state that " the production of work from heat is due, not to an actual consumption of caloric, but to the transport of caloric from a higher to a lower temperature." Carnot's analogy of the water-wheel is thus exactly justified. Caloric, like water, is not itself motive power, but is capable of performing work under suitable conditions in virtue of its tendency to flow down a temperature gradient. The quantity of motive power produced is directly proportional to the quantity of caloric and to the fall of temperature.

9. *The Motive Power of a Quantity of Caloric.*

The solution thus found by Carnot, and expressed in equation (5), was not merely the correct final solution of the problem on the caloric theory, it is also immediately obvious that it is perfectly consistent with the kinetic theory of heat, and supplies the master key to the relations between heat and motive power. Caloric may be said to possess motive power in virtue of its elevation of temperature, just as water possesses motive power in virtue of its available head or pressure. In the case of caloric there is a definite limit set by the absolute zero of temperature to the quantity of motive power obtainable by a cyclical process from a quantity of caloric supplied at any temperature. Putting $T_o=0$, the absolute zero, the maximum quantity obtainable from a perfect gas as working substance, and therefore from any other substance whatever, is evidently AQT, which may be regarded as the absolute work-value of a quantity of caloric, Q, supplied at a temperature, T, on the absolute scale. The quantity of motive power developed, if the fall available is $T-T_0$, is $AQ(T-T_0)$. The limit of efficiency for such a fall, or the ratio of the motive power obtained to the total motive power of the caloric supplied, is $(T-T_0)/T$. The motive power of the caloric rejected at T_0 is AQT_0. The sum of the quantities of motive power utilised and rejected is equal to the total motive power of the quantity supplied at T.*

Carnot's solution is not merely consistent with the mechanical theory; it directly states the principle of the conservation of motive power, and gives all the results which follow from that theory so far as they relate to reversible processes. He could not fairly have been expected to realise this at the time and to express his results in terms of the mechanical theory, because that theory had not been put in a definite form. His results are not necessarily incorrect because they were expressed in terms of the caloric theory. The numerical values which he calculated in various ways for the motive power of caloric were correct so far as the experimental data available at that time permitted. They deserve in reality to be regarded as the first calculations of the mechanical equivalent of heat, although they were expressed in a manner which renders them at first sight unfamiliar to modern readers.

10. *Carnot's Numerical Verification of his Principle.*

The investigation of the specific heats of gases having failed, on account of discordant experimental data, to lead to a *conclusive* result with regard to the variation of $F'(t)$ with temperature, Carnot next devotes his attention to the numerical calculation of the values of $F'(t)$ at different temperatures from the properties of different gases and vapours, so far as the scanty material at his disposal permitted. His first calculation from the properties of air at 0°C. appears at first sight to be unnecessarily long and involved, owing to his elaborate endeavour to avoid what he calls algebraic language; but he probably deduced the answer directly in the first instance from his expresison for the heat absorbed in isothermal expansion, applied to the difference of the specific heats—namely, $R/TF'(t)$. Employing his data, the value of $F'(t)$, or the work done in a cycle per degree fall per unit of heat supplied, comes out 1·395 kilogrammetres per kilocalorie per degree fall at 0°C. This method of calculation, based on the work done in a cycle, does not, like that of Mayer or Clausius, involve the assumption that the *whole work* done in isothermal expansion is equivalent to the heat supplied, which is true only for a perfect gas. Carnot's method applies to any gas or vapour, however imperfect, and would give the same result exactly for all substances at the same temperature if the experimental data were correct. The absolute equivalent of the kilocalorie is obtained, as explained in the last section, by multiplying this result by the absolute temperature corresponding to 0°C. on the scale of a perfect gas—namely, 273—which gives 380 kilogrammetres for the mechanical equivalent of the kilocalorie. The discrepancy from the true value, 427 kilogrammetres per kilocalorie, is due merely to errors in the experimental data.

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* This result, which is equivalent to the fundamental postulate of the mechanical theory, may readily be extended to a reversible cycle of any form. The given cycle may be supposed to be divided by a family of adiabatic or equicaloric curves into a number of elementary Carnot cycles of different ranges. For each of these elementary cycles, the quantity of caloric absorbed is equal to that rejected, and the difference of the energies of the caloric supplied and rejected is equal to the external work done. Taking the sum of the elementary cycles, the total quantity of caloric absorbed must be equal to that rejected in any cycle, which is the fundamental postulate of the caloric theory; and the difference of the quantities of heat energy absorbed and rejected in the cycle must be equal to the external work done, which is the fundamental postulate of the mechanical theory. The two postulates, so far from being opposed to each other, are mutually equivalent, and both are implied in Carnot's solution.

For steam at 100°C., taking Dalton's value of (dp/dt), the rate of increase of pressure with temperature—namely, 26 mm. of mercury, or 0·36 metre of water—and taking the specific volume of steam at 100°C. as 1,700 litres per kilogramme, Carnot finds the motive power dW/dt per degree fall in a cycle employing 1 kg. of steam, to be $1{,}700 \times 0{\cdot}36 = 611$ kilogrammetres. The quantity of heat supplied is the latent heat of vaporisation per kilogramme at 100°C., which he takes as 550 kilocalories. The work in kilogrammetres per kilocalorie per degree fall, or the value of $F'(t)$ is

$$F'(t) = 611/550 = 1{\cdot}112 \text{ at } 100^\circ\text{C}.$$

Multiplying this result by the absolute temperature 373°, we obtain 415 kilogrammetres as the absolute equivalent of the kilocalorie, which is a better approximation, because the data for steam were more accurate than those for air.

The method of calculation employed for steam is equivalent to the formula

$$(v—w)\ (dp/dt)=\mathrm{L}F'(t), \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where $(v—w)$ is the change of volume on vaporisation, and L the latent heat. This formula is generally known as Clapeyron's, but Carnot appears to be entitled to some credit for it, because it is simply the direct expression of his principle as applied to change of state, and because he was the first to employ it for numerical calculation, although he does not happen to give the formula itself in algebraic form in his footnotes.

Applying the same calculation to the vapour of alcohol at its boiling-point, 78·7°C., Carnot found the value $F'(t)=1{\cdot}230$ kilogrammetre per kilocalorie per degree fall. This would not necessarily agree with the value found from steam at 100°C., because the temperature was different. Assuming, in accordance with Watt's law, that the total heat of steam reckoned from 0°C. was constant and equal to 650 kilocalories per kilogramme, the latent heat at 78·7°C. would be 571 kilocalories. Taking the appropriate value of (dp/dt) from Dalton's tables, Carnot thus finds $F'(t)=1{\cdot}212$ from steam at 78·7°C., which agrees very fairly with the result deduced from alcohol at the same temperature. A similar calculation for steam at 0°C., taking the latent heat as 650 from Watt's law, gave the value $F'(t)=1{\cdot}290$, which differs materially from the value 1·395 found for air at the same temperature; but the data for steam were here so uncertain, that no stress could be laid on the discrepancy. The value $F'(t)=1{\cdot}290$ gives 352 kilogrammetres for the kilocalorie. The discrepancy in the values of the kilocalorie deduced in this way from steam at different temperatures is not to be wondered at, because Lord Kelvin ("Trans." R.S. Edn., XIV., 1849) in re-calculating by a modification of Carnot's method the mechanical equivalent J from Regnault's greatly improved data for steam, found values ranging from 1,357 ft.-pounds per pound degree C. at 0°C. to 1,578 ft.-pounds at 230°C. Joule had then by direct experiment found values ranging from 1,368 to 1,476 ft.-pounds at 10°C. to 16°C. Lord Kelvin was not satisfied that the value of J was the same at different temperatures. It required a bold speculator like Clausius to argue that there could be no variation with temperature.

Carnot laments that data were lacking, especially the values of (dp/dt), to enable him to calculate $F'(t)$ for solids and liquids, and particularly for the case of fusion of ice. If he had been in possession of Lord Kelvin's value—namely, 130 atmospheres—of the pressure required to lower the freezing point of water 1°C., he would have found $F'(t)=1\cdot53$, or 418 kilogrammetres as the equivalent of the kilocalorie at 0°C. With the data at his disposal, one can hardly wonder that he was baffled at every turn in his attempts to verify his theory.

Reviewing the numerical data for $F'(t)$, which varied from 1·112 kilogrammetres per kilocalorie per degree fall at 100°C. to 1·290 and 1·395 at 0°C., Carnot was led to the conclusion that $F'(t)$ probably diminished with rise of temperature on the gas scale, but that the numerical data were not sufficiently exact to determine the law of variation with temperature. Unfortunately the apparent diminution of $F'(t)$ with rise of temperature was most readily explained by supposing that the specific heat of a gas was not independent of the volume, but increased with increase of volume, as Delaroche and Bérard had found. We see immediately from Carnot's equation (4) that if v is greater than v_o, and U greater than U_o, then $F'(t)$ must be less than $F'(t_o)$. In other words, $F'(t)$ must diminish with rise of temperature. It happened that the mistake made by Delaroche and Bérard was very nearly of the right order of magnitude to account for the observed rate of diminution of $F'(t)$ with rise of temperature. Carnot was thus dissuaded from pursuing further the simple and obvious solution—namely, that $F'(t)$ was constant—which he had founded on the assumption that the specific heats were independent of the pressure.

If Carnot had not been misled by this fundamental error in the work of Delaroche and Bérard, and if his numerical data had been a little more accurate, he could not have failed to notice that the quantity of caloric in a kilocalorie varied inversely as the absolute temperature, or that its equivalent motive power was the same at all temperatures. In other words, that the kilocalorie was a unit of motive power, and not a unit of caloric. According to his data, neither the work obtainable per degree fall, nor the total work obtainable from a kilocalorie, was the same at different temperatures. Both appeared to vary to about the same extent, but in opposite directions. The point which naturally did not occur to Carnot, and which the experimental data were too uncertain to suggest, was the extremely fundamental point (which was also missed by subsequent writers in dealing with the caloric theory) that a quantity of caloric as measured by his equation, did not remain constant under the conditions of an ordinary calorimetric experiment, when heat was allowed to flow directly from a hot body to a cold body.

11. *Measurement of Caloric.*

According to Carnot's solution, $W=AQ(T-T_o)$, the natural measure of a quantity of caloric is the work done per degree fall in a Carnot cycle.

The constant A in this relation may be reduced to unity by a suitable choice of units. The absolute unit of caloric is that quantity which is capable of doing unit work per degree fall in a Carnot cycle. If the absolute unit of work is taken as the joule or watt-second, the absolute unit of caloric may appropriately be styled the CARNOT.

The CARNOT is that quantity of caloric which is capable of producing one joule of work in a Carnot cycle per 1°C. fall on the scale of a perfect gas.

The number of carnots of caloric required to vaporise one gramme of water at 100°C. under standard conditions is readily calculated from Carnot's data. Since the work done per gramme vaporised per degree fall is 0·611 kilogrammetres, or nearly 6 joules, the caloric of vaporisation is nearly 6 carnots. The work done per degree fall per kilocalorie of heat supplied at 100°C. is 1·112 kilogrammetres, or nearly 11 joules. The number of carnots of caloric in a kilocalorie at 100°C. is nearly 11. Taking 130 atmospheres C.G.S. as the pressure required to lower the melting point of ice 1°C., and 0·092 cubic cm. as the diminution of volume per gramme on melting, the work obtainable from 1 gramme of ice at 0°C. in a Carnot cycle per degree fall is $130 \times 0{\cdot}092/10 = 1{\cdot}2$ joules. The caloric of fusion is, therefore, 1·2 carnots. The mechanical equivalent of Q carnots supplied at T Abs. is QT joules. These values are calculated from the work which might be done in a reversible cycle, and are quite independent of calorimetric data. The caloric absorbed by 1 gramme molecule of a perfect gas ($R=8{\cdot}3$ joules per 1 deg.) in expanding at constant temperature from v_o to v, is given by Carnot's equation (3) as $Q=8{\cdot}3 \log_e (v/v_o)$ carnots, and is independent of the temperature. The difference of the specific capacities for caloric at constant pressure and volume at a temperature, T, is $8{\cdot}3/T$ carnots per degree or the absolute work value of the difference is 8·3 joules per degree.

12. *Equivalence of the Caloric and Kinetic Theories.*

The exact correspondence between the solution obtained by Carnot on the caloric theory with that subsequently obtained by Clausius, Rankine and Kelvin on the kinetic theory, is most easily appreciated if we compare the steps by which each solution was obtained. For this purpose we may follow the exposition of the kinetic theory as given by Clausius, as it is the most direct, and corresponds most nearly with the method of Carnot. Clausius starts with assuming Carnot's relation in the form $dW/dt = QF'(t)$, which is equally true on either theory, whether heat is measured as energy or as caloric. The divergence begins with the assumption that heat is measured either as a quantity of caloric (denoted by Q)*, or as a quantity of energy (denoted by q). Adopting the gas scale of temperature, these assumptions determine the form of the function $F'(t)$.

Carnot shows, as already explained, that if the specific heat of a gas at constant volume is independent of the pressure, $F'(t)$ must be constant, $=A$, on the caloric theory, if temperature is measured on the gas scale. This gives $dW/dt = AQ$, or $W = AQ(t - t_o)$ in a finite cycle. The measure of caloric is work per degree fall, and the work equivalent of caloric is found by multiplying the caloric by the absolute temperature and by a constant A. It follows that the work done by a perfect gas in isothermal expansion is the equivalent, AQT, of the caloric supplied, and that the difference of the specific capacities for caloric R/AT, is the same for all gases if equal volumes are taken.

Clausius proceeds in the opposite direction. He begins by assuming with Mayer as highly probable on general grounds that the work done by a gas in isothermal expansion is the equivalent Jq of the heat absorbed, which gives immediately, from Carnot's equation, $F'(t) = J/T$ when heat is measured as a quantity of energy. This had been previously suggested by Holtzmann (1845), but Clausius, having the advantage of Joule's experiments, was able to show that the numerical values of J/T were all reasonably consistent with Carnot's values of $F'(t)$ for the calorimetric unit. Applying this assumption to gases, it follows that the specific heat at constant volume must be independent of the pressure, and that the difference of the specific heats must be equal to R/J. Clausius ignores the evidence of Delaroche and Bérard on the variation of the specific heat with pressure, which gave Carnot so much trouble, but adopts that of Gay Lussac and Welter on the constancy of the ratio, which he (erroneously) states "cannot be reconciled with the theory of Carnot as heretofore treated." Assuming the constancy of the specific heats, he deduces Poisson's equation for the adiabatics. He finally obtains for the heat absorbed by a gas in isothermal expansion from v_o to v the expression $q = \mathrm{RT} \log_e (v/v_o)/\mathrm{J}$, which is the same as the assumption with which he started, and is equivalent to Carnot's expression (3). The results thus obtained were not new, except in the manner of expression. They were given to show that the kinetic theory was consistent with the then ascertained properties of gases.

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* To indicate that a quantity of heat is measured differently in the two sets of formulæ, capitals will be used for quantities depending on the caloric measure of heat, and small letters for the corresponding symbols in energy measure.

We have already seen that Carnot's principle takes the form given in equation (6) when applied to vapours. Substituting $\mathrm{F}'(t) = \mathrm{A}$, this becomes $(v - w)(dp/dt) = \mathrm{AL}$, where L is the latent caloric. On the kinetic theory we must substitute $\mathrm{F}'(t) = \mathrm{J}/\mathrm{T}$, and the equation becomes $(v - w)\,(dp/dt) = \mathrm{J}l/\mathrm{T}$, where l is the latent heat energy, and is equal to the latent caloric L multiplied by T, if the units are chosen so that the constants A and J are the same. Assuming J constant, Clausius employs this equation (in the reverse way to Lord Kelvin) to calculate the deviations of steam from the ideal state, which he regards as being the explanation of the variation of J with temperature deduced by Lord Kelvin from Regnault's experiments. This calculation was well meant, but indecisive, because, as we now know, Regnault's formula for the latent heat was erroneous.

Joule, by his experiments on gases, had arrived at the conclusion that in the performance of a heat engine the heat energy converted into work was not given back to the condenser. This is true of heat measured as energy, and appeared at first not only to be a conclusive disproof of the caloric theory, but also to be quite irreconcilable with the proof of Carnot's principle as given by Clapeyron. Clausius showed that this result was not necessarily inconsistent with Carnot's principle itself; but that, on the kinetic theory, in any cyclical process, the heat returned to the condenser must be less than that taken from the source by a quantity equivalent to the external work done. Applying this principle to an infinitesimal cycle with steam as the working substance, he deduced a new and unexpected result, discovered about the same time by Rankine. If saturated steam is compressed, heat must be abstracted from it to reduce it to the state of saturation at the higher pressure and temperature; in other words, the specific heat of steam maintained in the state of saturation is negative. This result is so good an illustration of the difference between the kinetic and caloric theories that it deserves fuller explanation.

If s', s'' are the saturation specific heats of water and steam respectively, l and $l+dl$ the latent heats of vaporisation at neighbouring temperatures T, and $\mathrm{T}+d\mathrm{T}$, the heat supplied in raising unit mass of water from T to $\mathrm{T}+d\mathrm{T}$ and vaporising it at $\mathrm{T}+d\mathrm{T}$ will be $s'd\mathrm{T}+l+dl$. The heat abstracted in cooling the saturated steam to T, and condensing it at T, will be $s''d\mathrm{T}+l$. The difference of the quantities of heat absorbed and abstracted—namely, $(s'-s'')d\mathrm{T}+dl$—must, according to the kinetic theory, be equivalent to the work done in the cycle $(v-w)\,(dp/dt)d\mathrm{T}$. But this work by the previous equation (6) deduced from Carnot's principle, is itself equivalent to a quantity of heat $(l/\mathrm{T})d\mathrm{T}$. Dividing the equation by $d\mathrm{T}$, so as to obtain the difference in the quantities of heat absorbed and abstracted per degree range of the cycle, we obtain the well-known equation of Clausius,

$$s'-s''+dl/d\mathrm{T}=l/\mathrm{T}. \quad . \; . \; . \; . \; . \; . \quad (7)$$

Knowing the approximate vlaues of all the quantities except s'' at 100°C.—namely, $s'=1$, $dl/d\mathrm{T}=-0{\cdot}70$, $l/\mathrm{T}=540/373=1{\cdot}45$ calories per degree—we obtain with Clausius, $s''=s'+dl/d\mathrm{T}-l/\mathrm{T}=-1{\cdot}15$, or the specific heat of saturated steam 100°C. is negative and numerically greater than that of water. Clausius remarks that this is inconsistent with the caloric theory, which gives the formula $s''=s'+dl/d\mathrm{T}$, without the negative term l/T, and would make the specific heat *positive* and equal to $+0{\cdot}30$, according to Regnault's data. In reality there is no inconsistency. It is merely a question of the difference between the caloric measure and the energy measure of heat.

On the caloric theory the corresponding equation is deduced as follows by Carnot's method. If S′, S″ are the specific capacities for caloric of water and steam in the state of saturation, and if L and $\mathrm{L}+d\mathrm{L}$ are the values of the latent caloric of vaporisation at T and $\mathrm{T}+d\mathrm{T}$, the caloric absorbed in the cycle—namely, $\mathrm{S}'d\mathrm{T}+\mathrm{L}+d\mathrm{L}$—must be equal to the caloric abstracted—namely, $\mathrm{S}''d\mathrm{T}+\mathrm{L}$—we thus obtain

$$\mathrm{S}''=\mathrm{S}'+d\mathrm{L}/d\mathrm{T}, \quad . \; . \; . \; . \; . \; . \; . \quad (8)$$

which agrees *in form* with the equation ascribed by Clausius to the caloric theory, but the quantities involved are quantities of caloric, and not quantities of energy. If we imagine s', s'' and l, expressed in joules, and S', S'' and L in carnots, the latter are reduced to joules by multiplying them by T. Mulitiplying through the last equation by T, and writing $S''T=s''$, $S'T=s'$ and $L=l/T$, we obtain

$$s''=s'+Td(l/T)/dT=s'+dl/dT—l/T, \quad . \quad . \quad . \quad (9)$$

which is *identical* with the equation deduced by Clausius from the kinetic theory.

We deduce from the above example, or from many others which might be given, that the fundamental postulate of the caloric theory, which Carnot employed in deducing his solution $W=AQ(T—T_0)$, and which Clausius was regarded as having conclusively disproved—namely, that the caloric absorbed is equal to that abstracted in any reversible cycle—so far from being opposed to the kinetic theory, is mathematically equivalent to Joule's proposition that the heat energy absorbed exceeds that abstracted by the equivalent of the work done.

It is surprising that Clausius in his first Paper did not give the complete solution for a Carnot cycle of finite range, which follows immediately from his assumption $F'(t)=J/T$ on the kinetic theory. Putting $H=Jq$ for the heat absorbed at T, measured in work units, and observing that dH, the difference between the quantities of heat absorbed and abstracted, is equal to dW the work done in the cycle, Carnot's equation $dW/dt=qF'(t)$ reduces to the form, $dH/dt=H/T$. From which it immediately follows that the ratio H/T of the heat H taken in at T to the temperature T in a finite cycle of range T to T_0, is the same as the ratio H_0/T_0 of the heat rejected H_0 to the temperature T_0 at which it is rejected. Since $H—H_0=W$, the work done in the cycle, the solution may be written in the familiar form,

$$H/T=H_0/T_0=W/(T—T_0), \quad . \quad . \quad . \quad . \quad (10)$$

which is precisely equivalent to Carnot's solution, $Q=Q_0=W/(T—T_0)$.

The solution in something approaching very nearly to this form appears first to have been given by Rankine, without proof, in a footnote to his Paper on the Mechanical Theory of Heat (" Phil. Mag.," Ser. IV., Vol. II., p. 65, 1851). He gives " the maximum value of the fraction of the whole heat converted into expansive power " as $(T—T_0)/(T—k)$, " where k is a constant, the same for all substances." which is apparently intended to take account of the difference between the absolute zero of the gas thermometer, and the "absolute zero of heat." Lord Kelvin ("Phil. Trans.," 1854) subsequently proceeded by the inverse process. Instead of adopting the scale of a perfect gas, as assumed by Carnot, he defined absolute temperature, by the above relations (10), in such a way as to agree with the perfect gas scale; and proceeded to investigate, with Joule's assistance, the deviations of actual gases from the scale so defined.

13. *Reappearance of Caloric in the Mechanical Theory.*

Since Carnot's equation, $dW/dt = QF'(t)$, was adopted without material modification into the mechanical theory, so that $QF'(t)$ remained simply a quantity of Carnot's caloric (though Q was measured in energy units and $F'(t)$ received the appropriate value J/T required to reduce energy units to caloric), it was inevitable that caloric should make its reappearance sooner or later in the mechanical theory. It first appears, disguised as a triple integral, in Kelvin's solution ("Phil. Mag.," IV., p. 305, 1852) of the problem of finding the work obtainable from an unequally heated body. The solution (as corrected later) is equivalent to the statement that the total quantity of caloric remains constant when the equalisation of temperature is effected by means of reversible engines. Caloric reappears next as the "thermodynamic function" of Rankine, and as the "equivalence-value of a transformation" of Clausius ("Pogg. Ann.," XCIII., p. 497, 1854). Finally, in 1865, when its importance was more fully recognised, Clausius ("Pogg. Ann.," CXXV., p. 390) gave it the name of "entropy," and defined it as the integral of dQ/T. This definition depends on the calorimetric or energy measure of heat, and obscures the fact that the caloric measure of heat follows directly from Carnot's principle, and may be made independent of the calorimetric measure. No one at that time appears to have appreciated Carnot's solution, or to have realised that entropy was merely caloric under another name. In justice to Carnot, it should be called caloric, and should be defined directly by means of his equation $W = AQ(T - T_0)$. This method of procedure appears to be justifiable both logically and historically, and leads to a more practical and definite conception of entropy or caloric as the true measure of a quantity of heat as opposed to a quantity of thermal energy. The mathematical definition of entropy, as the integral of dQ/T under certain restrictions, is unintelligible to the average student, for whom the conception of entropy possesses an artificial atmosphere of unreality. The conception of caloric defined by Carnot's equation would be more readily assimilated, and might be introduced at a much earlier stage. The direct investigation of the properties of caloric thus defined would probably lead to a more general and intimate appreciation of Carnot's principle itself and of many modern developments of thermodynamics which are practically beyond the comprehension of the majority of students under the present system.

14. *Caloric in Irreversible Processes.*

Carnot was the first to distinguish between reversible and irreversible processes in thermodynamics, and to lay down the simple and sufficient rules for the reversibility of a heat engine. (1) Such an engine must work without friction, which involves direct waste of mechanical power, and had long been recognised as the non-conservative element in mechanical systems. (2) There must be no direct passage of heat from a hotter to a colder body, which is the criterion of thermal reversibility. In order to complete the caloric theory of heat, it was necessary to find an answer to the questions: (1) What becomes of motive power spent in friction? and (2) What happens to caloric when it passes directly from a body at a higher to a body at a lower temperature?

Rumford and Davy had proved beyond cavil that caloric was generated by friction. Carnot (as we see from the notes published by his brother) had already, before his early death in 1832, arrived at the general theory of the equivalence of heat and motive power, and had projected a number of experiments in which the motive power consumed should be measured at the same time as the heat produced. From the purely theoretical side it would have been most natural to assume that the absolute motive power of the caloric generated was equal to the motive power consumed, as given by the equation $W=AQT$, for the maximum amount of motive power obtainable from a quantity Q of caloric at T, which with A for $F'(t)$ is the same as Carnot's expression for the heat evolved in compressing a perfect gas. We know now that this is the correct solution. But Carnot, being aware of the conflicting nature of the experimental evidence, felt that further experimental verification was necessary, which was first afforded by Joule.

Carnot showed that the direct passage of heat from a hotter to a colder body was equivalent to wasting a difference of temperature which might have been utilised for the production of motive power. It was natural to ask what became of the motive power wasted. In the analogous case of the waterfall, the answer is fairly obvious. If the water is allowed to fall freely, its motive power is expended in the production of *vis viva* equivalent to its fall, which is converted into heat by friction when its motion is arrested. Similarly, when a compressed gas is allowed to expand into a vacuum, the work which might have been obtained from it by reversible expansion in a cylinder provided with a piston, is spent in producing *vis viva* of the issuing current of gas, which is finally converted into heat by friction, so that the quantity of caloric in the gas is increased, instead of remaining constant as in reversible adiabatic expansion. Carnot considers a case of this kind in a footnote. Gay Lussac and Welter had observed that if a small opening were made in a large reservoir of compressed air, and the bulb of a thermometer were presented to the issuing current, there was no sensible lowering of temperature observed, such as would have been produced in a similar reversible expansion. Carnot attributes this result partly to the development of caloric by friction against the sides of the orifice, and partly to the increase of pressure close to the thermometer bulb due to the impact.

When a quantity of caloric, Q', supplied at a temperature, T', is allowed to fall directly by conduction to a temperature T'', without producing the equivalent motive power $AQ'(T'—T'')$, which might have been obtained from the fall $(T'—T'')$ by means of a reversible engine, it is not so immediately obvious, owing to our ignorance of the mechanism of conduction, what becomes of the motive power wasted. The case is analogous to the fall of a quantity of electricity in a conductor, through a difference of potential in the conductor, in which case the caloric generated is equivalent to the motive power wasted. On Weber's hypothesis the carriers of heat and electricity in metals are the same, and the methods by which caloric is generated in either case must be closely related. It would be reasonable to assume by analogy that a quantity of caloric, $Q'(T'—T'')/T''$, equivalent to the wasted motive power at the lower temperature, was generated in the fall by some process analogous to friction. Adding this to the original quantity Q', we find the quantity Q'' of caloric recovered at the lower temperature T'' equal to $Q'T'/T''$. In other words, the quantity of caloric is increased by falling freely in such a way that its equivalent motive power $AQ''T''$ at the lower temperature is the same as that which it originally possessed—namely, $AQ'T'$—at the higher temperature. This is precisely what we now assume on the analogy of material systems in applying the principle of the conservation of energy to heat. Carnot appears to have foreseen this result, and to have devised experiments for verifying it, but so far as I know it has not been explicitly verified, merely because it has not occurred to anyone to define caloric explicitly in terms of work per 1 deg. fall in a cycle, and to compare this measure directly with the calorimetric units.

At the present time so many data exist for the verification of the increase of caloric in an irreversible process, that it is unnecessary to perform a special experiment. The inevitable result of such an experiment may be predicted by a simple calculation, such as the following. We have already seen that the caloric of vaporisation of steam at 100°C., measured in terms of Carnot's equation, is approximately 6 carnots per gramme, and the caloric of fusion of ice at 0°C. is 1·2 carnots per gramme. Arrange an experiment in which steam is condensed at 100°C. on one side of a conducting partition, while ice is melted at 0°C. on the other side, without allowing any heat to escape. Measure both the steam condensed and the ice melted in a given time. We know, as the result of innumerable calorimetric experiments of this kind, that for each gramme of steam condensed, 540/79·5 grammes of ice approximately would be melted. Six carnots at 100°C. when allowed to fall directly to 0°C. by conduction, produce $540 \times 1{\cdot}2/79{\cdot}5 = 8{\cdot}17$ carnots at 0°C. The quantity of caloric at 0°C. is greater than that supplied at 100°C. in the proportion of 8·17 to 6, which is nearly the same as the ratio 373/273 of the absolute temperatures. The motive power of the caloric at 100°C.—namely, $6 \times 373 = 2{,}238$ joules—is the same as the motive power of the caloric found at 0°C—namely $817 \times 273 = 2{,}231$ joules—within the limits of error of the data. Similarly in other cases, whenever available motive power is wasted "in the useless re-establishment of the equilibrium of caloric," an equivalent quantity of caloric is generated, so that the total motive power, including any useful work done, remains constant. In a reversible cycle, when there is no waste and the efficiency is a maximum, the total quantity of caloric remains constant. The increase of caloric, if any, due to friction or conduction, is a measure of the motive power wasted.

Caloric is not conserved in the old sense that its total quantity remains constant in any system of exchanges under all conditions. It remains constant only in the restricted sense, provided that all the exchanges are reversible or take place under conditions of equilibrium. But since in any irreversible exchanges the total quantity of caloric is always increased, the principle of the conservation of caloric can still be stated in the modified form that " the total quantity of caloric in a system *cannot be diminished* by any internal reactions or by the performance of work by or on the system, provided that no heat is allowed to escape." Applied in this form, with various modifications to suit special conditions, it has proved to be one of the most fruitful principles of modern thermodynamics ; but it might have been reached sooner, and more generally appreciated, if the caloric theory had not been so hastily abandoned.

14. *The Calorimetric Measure of Heat.*

The only defect of the caloric theory as developed by Carnot lay in the tacit assumption that the ordinary calorimetric units were units of caloric. This, no doubt, was one of the fundamental points in the theory of heat, which, in Carnot's opinion, " required the most attentive examination." He had himself supplied the key to the difficulty in his criterion of thermal reversibility. But no experimental data were available to decide the point. The pioneers of the mechanical theory made the same tacit assumption when they took it for granted that a given quantity of work done in compressing a gas at any temperature would produce the same number of units of heat in a calorimeter. Granting that heat was measured as energy in a calorimeter, and that energy was conserved, the required result followed ; but this was the very point which was to be proved. Even Joule's experiments were not sufficient in themselves to decide the point, because they were all performed at nearly the same temperature, so that it made no difference whether heat was measured as caloric or as energy. According to Carnot's equation (3), the ratio of the caloric evolved to the work done should be constant at any one temperature, and should vary comparatively slowly with the temperature. Lord Kelvin appears to have appreciated this point when he endeavoured to calculate, by means of Regnault's data for steam, the number of foot-pounds of work required to generate 1 calorimetric unit of heat by compressing a gas at different temperatures. The number varied from 1,357 to 1,578 foot-pounds per pound-degree C., but this result was not conclusively against the mechanical theory, because, as Clausius showed, it admitted a different interpretation. The first experiments which were sufficiently definite and accurate to decide the point were those of Regnault ("Phil. Mag.," V., p. 473, 1853), which showed that the specific heat of air, measured calorimetrically, was nearly independent of pressure and temperature over a wide range. It follows from Carnot's expression $R/TF'(t)$ for the difference of the specific heats, that if heat is measured calorimetrically, $F'(t)$ must be equal to J/T as required by the mechanical theory, assuming the ratio of the specific heats to be constant. But if Carnot's solution (deduced on the assumption that the specific heat of air was independent of the pressure) had not been overlooked, it would have been obvious that caloric itself was not energy, and that Regnault's result was not inconsistent with the measure of heat as caloric, but only with the tacit assumption that the caloric measure was identical with the calorimetric measure. If this had been recognised at the time, it would have been quite unnecessary to recast and revolutionise the entire theory of heat. Evolution would have proceeded along safer lines with the retention of caloric, and the investigation of its properties, which are so important in all questions of equilibrium in physics.

15. *Caloric the True Measure of Heat Quantity.*

Since a quantity of heat is measured most directly as a quantity of thermal energy in an ordinary calorimetric experiment, we have become so saturated with the idea that heat is energy and must be measured in units of energy, that we are apt to forget that a quantity of heat is not completely specified by its energy equivalent. The absurdity would be at once apparent, to take an analogous case, if we were to measure a quantity of electricity always in kilowatt-hours. The equivalent energy determines the cost of production, and is for many purposes the appropriate and sufficient measure, but we should fare very badly in electrical theory without the separate units of quantity in ampere-hours and pressure in volts. In electricity, the conditions of practical measurement have led us naturally to units of quantity defined in terms of electric and magnetic forces, and the unit difference of potential follows as that difference through which unit quantity must fall in doing unit work. In the science of heat Carnot's equation provides us with a precisely analogous measure of heat quantity or caloric as distinct from a quantity of energy. The unit of caloric is that which does unit work in falling reversibly through 1 deg. of temperature. The unit of caloric might be defined directly in terms of the expansion of a gas at constant temperature by the equation $Q=R \log_e (v/v_0)$, in which case the scale of temperature would follow. But since this method would be inconvenient for the practical measurement of caloric, we define the temperature scale first, as that of the perfect gas, and deduce the natural unit of heat quantity, which comes to the same thing. It is true that we can solve most questions in heat in terms of energy and temperature, without explicit reference to caloric or its equivalent. We could similarly solve most electrical problems without mentioning amperes. But since caloric possesses the important property, essential to the natural measure of heat quantity, of remaining constant in reversible exchanges (which the energy measure of heat does not) all our equations and methods of reasoning with regard to questions of equilibrium are greatly simplified and rendered more direct if we adopt caloric as the true measure of heat quantity, and regard it as possessing energy in virtue of its temperature. With all our preconceived notions of heat as a "form of energy," it is difficult now to retrace our steps and express everything in terms of caloric. But, if Carnot's solution had not been overlooked 60 years ago, it would have seemed equally absurd to regard a quantity of heat merely as a quantity of energy, and we should probably have been far ahead of our present position.

16. *The Material Nature of Caloric.*

The objection most commonly urged against the caloric theory from the earliest times has always been that it was absurd to suppose that anything which could be generated without limit could possibly be regarded even in thought as a material fluid. Some 20 years ago, the fluid theory of electricity was regarded as being equally illogical and physically unsound, although it was generally retained on account of the many useful analogies which it suggested. A natural reaction, consequent on fresh discoveries, has caused the pendulum to swing far in the opposite direction. Electricity is now more material than ever, and is continually invading other branches of physics with materialistic notions. So long as a quantity of heat is regarded as being necessarily a quantity of energy, the fluid conception is of little or no use. It is true that a quantity of energy cannot be created or destroyed, but since it may be transformed into an equivalent quantity of any other kind of energy, it cannot be identified as remaining constant in any one state. This objection does not apply to caloric. It is true that caloric can be created without limit, just as we think and speak of electricity as being generated; but it has the most important attribute that, when once generated, it can never, so far as we know, be destroyed. Moreover, caloric remains constant under precisely those limiting conditions of equilibrium which are most important for purposes of practical calculation. It is true that it may be difficult to isolate a particular set of material particles and label them caloric. It is conceivable that caloric may not be material at all, but merely, as Boltzmann puts it, "the logarithm of the probability of a complexion." But if this is really the case, it is all the more necessary for our sanity and progress to think and speak of it as a material fluid. Although this conception of caloric may appear at first sight to run counter to some of our most cherished popular illusions with regard to heat, there could be no serious objection to adopting it as a convenient method of expression. The more shadowy the conception to be visualised, the greater the need of a definite material analogy. From this point of view the old picturesque phraseology of the material fluid, implied in Carnot's waterfall, so far from being a valid objection, is one of the chief advantages of the caloric theory.

17. *Conclusion.*

The modes of thought and expression inherent in the caloric theory lead most naturally and inevitably to the conception of the conservation of caloric as the fundamental law peculiar to thermodynamics. To the mathematician, who loves to deal in abstract mysteries, the substitution of the crude idea of caloric for the esoteric conception of entropy may doubtless prove repellent; but to the experimentalist, who prefers to think in concrete realities, the change of view point cannot fail to be suggestive. So long as entropy was merely a mathematical abstraction it was unnecessary to attach any definite meaning to the absolute quantity of entropy in a body, and no one objected to its being infinite when reckoned from zero pressure, temperature, or volume. But if we regard caloric as the natural measure of heat quantity, and push the conception to its logical conclusion, the absolute quantity of caloric in a finite body must be finite, and must vanish at the absolute zero of temperature. The specific heats in energy measure, instead of remaining constant or tending to a finite limit, as generally assumed, must tend to vanish as the zero is approached. The ideal gas of constant specific heat is an obvious objection to this rule, but the real gas avoids the difficulty by condensing in good time. This direct corollary of the caloric theory, so far from being inconsistent with experiment, is daily receiving confirmation from recent researches. It would be impossible, in a sketch like the present, to follow the caloric theory into all its possible developments, but enough has been said to show that the conception of caloric, as employed by Carnot, was not so misleading or erroneous as is generally supposed.

ABSTRACT.

The caloric theory of heat as developed by Carnot in his famous "Reflexions on the Motive Power of Heat" (Paris, 1824) leads immediately to the correct solution of the relations between heat and motive power (energy or work) in all reversible processes, and appears to be in some respects preferable to the mechanical theory as a method of expression, because it emphasises more clearly the distinction first clearly stated by Carnot, between reversible and irreversible transformations, and because it directly provides the natural measure of a quantity of heat as distinct from a quantity of thermal energy.

Carnot first introduced the method of the cyclical process in discussing the action of a heat engine, and showed that, in the ideal case, if there were no direct transference of heat between bodies at different temperatures, the transformations of heat and motive power in such a cycle were reversible. Assuming that it was impossible to imagine a heat engine capable of producing motive power perpetually without taking any heat from the boiler, he concluded that the quantity of motive power, W, produced from a given quantity of heat, Q, by means of a reversible engine, working between given temperature limits in a cyclical process, was the maximum obtainable; or that the efficiency must be independent of the agents employed, and must be a function of the temperature limits alone. He expressed this by the equation $W/Q=F(t)$, between finite limits 0° and t°C., or by the equivalent equation $dW/dt=QF'(t)$ for a cycle of infinitesimal range, dt, at a temperature, t, where $F'(t)$ (generally known as Carnot's function) is the derived function of $F(t)$, and must be the same for all substances at the same temperature.

Applying the equation in this form to a gas obeying the law $pv=RT$, he showed that the heat absorbed in isothermal expansion from v_0 to v

was given by the expression $Q = R \log_e(v/v_0)/F'(t)$, and that the difference of the specific heats at constant pressure and volume, given by the expression $S_p - S^v = R/TF'(t)$, must be independent of the pressure, and the same for equal volumes of all gases. These results were new, but were confirmed experimentally by Dulong five years later. Carnot showed, further, that if the ratio S_p/S_v was constant (as found by Gay Lussac and Welter, and assumed by Laplace and Poisson), both S_p and S_v must be independent of the pressure.

The results so far obtained by Carnot, including the description of his reversible cycle and the deduction of his fundamental principle, were independent of any assumption as to the nature of heat. Applying the assumption of the caloric theory, that the quantity of caloric required to change the state of a substance from (v_0, t_0) to (v, t) was the same by any reversible process, Carnot deduced that, if S_v was independent of the pressure, the function $F'(t)$ must be constant, $=A$. This assumes that heat is measured as caloric, and that temperature is measured on the scale of a gas, obeying the law $pv = RT$, and having S_v independent of the pressure, which is equivalent to the modern definition of a perfect gas. Putting $F'(t) = A$, he obtains for the work W produced from a quantity of caloric, Q, supplied at a temperature, T, in a cycle of finite range T to T_0, an expression equivalent to the following:—

$$W = AQ(T - T_0).$$

Carnot was unable to reconcile this solution with the imperfect experimental data available in his day, and particularly with the observation of Delaroche and Bérard, supported by Laplace's theory, that the specific heat of air, S_p, diminished with increase of pressure, which we know now, from the experiments of Regnault, to have been incorrect. He therefore made no serious attempt to apply the solution, and subsequent writers have apparently failed to observe that it is the correct final solution of the problem on the caloric theory. With our present knowledge, it is easy to see that this solution of Carnot's is also consistent with the mechanical theory, and contains implicitly all the relations of heat and work so far as they relate to reversible processes. The quantity, Q, of caloric remains constant in reversible expansion such as is postulated by Carnot, when no heat is supplied. The work done is directly proportional to the temperature range $T - T_0$. The absolute motive power or equivalent work-value of a quantity of caloric, Q, supplied at a temperature, T, is the maximum work obtainable from a perfect gas (and therefore from any other substance whatever) when $T_0 = 0$, namely, AQT. The efficiency of the cycle with range T to T_0 is $W/AQT = (T - T_0)/T$. The external work done in the cycle is the difference of the work-values of the caloric supplied and rejected, a result which is readily extended to cycles of any form.

To complete Carnot's solution, it is necessary to enquire what happens to caloric in irreversible processes, such as friction, or the direct passage of heat from a hotter to a colder body. Carnot, as we see from his posthumous notes, had already, before his early death in 1832, arrived at the general conception of the conservation of motive power, and had planned experiments in which the motive power consumed in friction should be measured at the same time as the caloric generated. According to his theory, it would have been natural to assume that the motive power of the caloric generated at any temperature, namely AQT, should be equal to the motive power consumed in friction. But he realised that further experimental evidence was necessary, which was first supplied by Joule.

A quantity of caloric is defined in Carnot's equation as measured by work done in a Carnot cycle per degree fall. The absolute unit of caloric, which may appropriately be called the CARNOT, is that quantity which is capable of doing one joule of work per degree fall. The mechanical equivalent of Q carnots at T Abs. is QT joules. From Carnot's data, the work done in a cycle per gramme of steam vaporised at 100°C. per degree fall is 0·611 kilogrammetres, or nearly 6 joules. The caloric of vaporisation is 6 carnots. Similarly, from Kelvin's data for the pressure required to lower the freezing point 1°C., the caloric of fusion of ice is 1·2 carnots. Since this definition is independent of calorimetric measurements, it may be employed in a calorimetric test, in which steam is condensed at 100°C. on one side of a conducting partition while ice is melted at 0°C. on the other, to determine by direct experiment what happens when caloric falls irreversibly by conduction from 100°C. to 0°C. We know that for each gramme of steam condensed, or for each 6 carnots supplied at 100°C., 540/79·5 grammes of ice approximately would be melted, or 8·17 carnots of caloric would appear at 0°C. The quantity of caloric is increased in the proportion 373/273. The motive power of the caloric remains constant if no useful work is done. The increase of the quantity of caloric is the same as if the available motive power $AQ(T-T_0)$ had been developed and converted into heat by friction at the lower temperature. Whenever motive power is wasted in friction, or "in the useless re-establishment of the equilibrium of caloric," a quantity of caloric equivalent to the wasted motive power is generated. The total quantity of caloric in an isolated system remains constant only if all the transformations are reversible, in which case the motive power developed exactly suffices to restore the initial state. In all other cases there is an increase of caloric. The old principle of the universal conservation of caloric, which is true only for reversible processes, must therefore be modified as follows :—"*The total quantity of caloric in any system cannot be diminished except by taking heat from it.*"

This principle, with various modifications to suit special cases (such as conditions of constant temperature, pressure, or volume) is immediately recognised as one of the most fruitful in modern thermodynamics. But it appeals more forcibly to the imagination of the student, if established, as roughly sketched above, by a direct investigation of the properties of Carnot's caloric.

The caloric theory is seen to be perfectly consistent with Carnot's principle and with the mechanical theory for all reversible processes. Caloric is the natural measure of a quantity of heat in accordance with Carnot's equation, if we adopt the gas-scale of temperature. The only defect of the caloric theory lay in the tacit assumption, so easily rectified, that the ordinary calorimetric units were units of caloric. The quantity measured in an ordinary calorimetric experiment is the motive power or energy of the caloric, and not the caloric itself. If this had been realised in 1850, it would have been quite unnecessary to recast and revolutionise the entire theory of heat. Evolution might have proceeded along safer lines, with the retention of caloric, and the investigation of its properties, which are of such fundamental importance in all questions of equilibrium in physics.

Since Carnot's equation, $dW/dt=QF'(t)$, was adopted without material modification into the mechanical theory, and $QF'(t)$ remained simply a quantity of Carnot's caloric (though Q was measured in energy units and $F'(t)$ received the appropriate value J/T required to reduce energy units to caloric) it was inevitable that Carnot's caloric should make its reappearance sooner or later in the mechanical theory. It first reappears, disguised as a triple integral, in Kelvin's solution (" Phil. Mag.," 4, p. 305, 1852) of the problem of finding the available work in an unequally heated body. The solution (as corrected later) is equivalent to the statement that the total quantity of caloric remains constant when the equalisation of temperature is effected reversibly. Caloric reappeared next as the " thermodynamic function " of Rankine, and the " equivalence-value of a transformation " (Clausius "Pogg. Ann.," 93, p. 497, 1854). Finally, in 1865, when its importance was more fully recognised, Clausius (" Pogg. Ann.," 125, p. 390) gave it the name of " entropy," and defined it as the integral of dQ/T. Such a definition appeals to the mathematician only. In justice to Carnot, it should be called caloric, and defined directly by his equation $W=AQ(T-T_0)$, which any schoolboy could understand. Even the mathematician would gain by thinking of caloric as a fluid, like electricity, capable of being generated by friction or other irreversible processes. Conduction of caloric is closely associated with the electrons, and the science of heat would gain, like the science of electricity, by attaching a more material conception to the true measure of a quantity of heat, as distinguished from a quantity of thermal energy.

A vote of thanks to Prof. CALLENDAR for his Presidential Address moved by Dr. CHREE and seconded by Dr. RUSSELL, was carried unanimously.

XV. *On the Movement of a Coloured Index along a Capillary Tube, and its Application to the Measurement of the Circulation of Water in a Closed Circuit.* By ALBERT GRIFFITHS, *D.Sc., Head of Physics Department, Birkbeck College.*

READ FEBRUARY 24, 1911.

- § 1. Introduction and elementary theory.
- § 2. Experiments with an index in a tube of 2 mm. bore.
- § 3. Details of index; methods of observation; and tests of consistency.
- § 4. Experiments related to the elementary theory.
- § 5. Applications.

§1. *Introduction.*

If a capillary tube, originally full of water, and along which a slow flow is taking place, be fed with a weak fluorescein solution, the point to which the colour reaches can be read, at the speeds studied in this Paper, with tolerable consistency when the illumination is constant; and as there is a paraboidal flow under certain conditions along a capillary tube the apex of the paraboloid moving at twice the mean speed of the water, a hasty consideration of the subject might lead to the erroneous conclusion that the extremity of the coloured index moves at twice the mean speed, or even more, the excess being due to diffusion.

At the speeds studied in this Paper, it is found that the extremity of the coloured index moves at a speed distinctly less than twice the average speed of the water; in fact, its movement is a rough measure of the average speed.

In this Paper the full mathematical treatment is not attempted; but an elementary consideration, although not complete, will be of advantage in dealing with the experiments. It can easily be shown that if the intensity of the colour were constant over a cross-section of the tube the colour would diffuse along the tube exactly as if the water travelled in a solid column. The intensity of the colour can not be absolutely constant over a cross-section except in the theoretical case of a capillary tube of infinitely small bore; but the experiments of §4 show that when the rate of flow is small the error involved in the assumption is not great. By stopping the supply of fluorescein solution and replacing it by water, an approximately symmetrical column of colour of slowly increasing length can be obtained, and when the slowly moving column is at a relatively long distance from the ends of the fine capillary, it is obvious that the movement of the centre of the column must measure the mean speed of the water.

But even when it is not correct to assume that the intensity of colour is absolutely constant over a cross-section, an approximately symmetrical column, the centre of which indicates the mean speed, can be obtained. This is proved in the next section.

§ 2. *Experiments with a Tube of about 2 mm. Bore.*

A convenient apparatus for the experimental study of the subject is shown in Fig. 1. T is a calibrated tube containing the coloured index, R is a series of fine tubes acting as a resistance, H is a tube containing water, the height of which regulates the rate of flow through the tube T, I is indiarubber tubing, S is a stop-cock, and B is a beaker containing water on which floats a layer of oil to minimise evaporation.

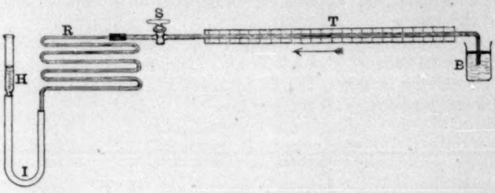

FIG. 1.

The following table of observations and calculations explains itself :—

Average readings of ends of index, 1 p.m., Nov. 19th	134·32, 144·31
Corrected readings	134·44, 144·22
Mean of corrected readings	139·33 cm.
Weight of beaker, water and oil, 1 p.m., Nov. 19th	53·6800 gms.
Average readings of ends of index, 11 a.m., Nov. 24th ...	52·25, 70·02
Corrected readings	52·38, 69·927
Mean of corrected readings	61·15 cm.
Weight of beaker, oil and water, 11 a.m., Nov. 24th	50·8232 gms.
Correction due to evaporation, obtained with aid of auxiliary beaker	0·0028 gm.
Corrected weight of beaker	50·8260 gms.
Corrected loss of weight of beaker	2·854 gms.
Product of movement (78·18 cm.) of index into mass (0·4964 ÷ 13·56) per linear centimetre	2·859 gms.
Percentage difference..	0·18
A second experiment gave :—	
Corrected loss of weight of beaker	3·8028
Product of movement (104·056 cm.) of index into mass (0·4964 ÷ 13·56) per linear centimetre	3·8099
Percentage difference	0·19

§ 3. *Details of Index, Methods of Observation and Tests of Consistency.*

The fluorescein solution employed in the experiments of this Paper was made by adding one quarter of a gram of fluorescein and two drops of an aqueous solution of ammonia of strength 0·88 to a litre of distilled water. The tube containing the index is placed on a black scale with white graduation marks.

When readings were taken directly with the eye the source of illumination was an arc lamp taking 12 amperes, placed in a lantern with only the condenser in position. A sheet of ground glass covered the condenser to diffuse the light and to give an even illumination of the index; and a sheet of blue glass placed in front of the ground glass absorbed a quantity of light without any appreciable diminution of the fluorescence of the index.

The consistency of the readings was tested in the case of a calibrated tube of about 0·25 mm. diameter, by introducing two indexes in series, the linear speed being of the order of 1·5 cm. per hour, or 36 cm. per day.

	Movement of front index. Centimetres.	Movement of back index. Centimetres.	Ratio of front to back.
1	55·39	55·00	1·0071
2	51·63	51·83	0·9960
3	58·27	58·22	1·0009
4	68·18	68·19	0·9999
5	68·25	68·33	0·9988
6	51·14	51·34	0·9961
7	55·94	56·21	0·9952
8	44·95	44·97	0·9996
9	52·48	52·08	1·0077
10	54·55	54·65	0·9981
11	57·47	57·39	1·0014
12	59·47	59·57	0·9983
		Average	0·999925

The probable error of a single value of the ratio, as given by the formula $r=0{\cdot}8453\sum v\div\sqrt{n(n-1)}$, where $\sum v$ is the sum of the arithmetical differences between the values of the ratios and 0·999925, is 0·002561—*i.e.*, about ¼ per cent.

The probable error of the arithmetical mean of the ratios, as given by the formula $r=0{\cdot}8453\sum v\div n\sqrt{n-1}$ is 0·0007392—*i.e.*, less than $\frac{1}{10}$ per cent.

On the assumption that all the indexes are equally reliable, whatever their ages, it can be proved that the probable error of an individual reading of the mean position of an index is

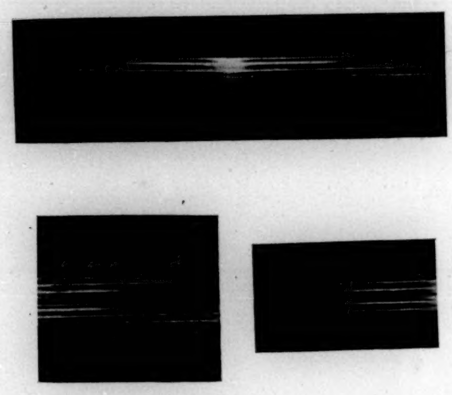

Fig. 2.
Velocity 1·25 cm. per hour; diam. min.; age 2 days.

To face p. 193.

0·7 mm. The assumption is not correct; nevertheless, the value 0·7 mm. gives a rough idea of the order of accuracy when no special devices are employed to obtain an exact reading. It must be mentioned that as a rule only one observation of each end of an index was taken.

After the preceding 12 experiments were concluded, an attempt was made to determine the consistency of the reading of the middle point of the index by making two parallel white lines on a strip of black paper at a distance apart approximately equal to the length of the index. The strip of paper was adjusted until the two white lines occupied a symmetrical position with respect to the index. Mr. H. G. Bell, who is responsible for nearly all the readings of this Paper, took six readings with the aid of the paper strip. The probable error calculated from the six readings came out to be 0·36 mm.

A photographic method of taking the readings has been developed which possesses some features of interest. The source of light is the same as that employed for the eye-readings, excepting that the blue glass is removed. Panchromatic plates are used in the camera, with a screen of "Rapid Filter Green." The arc is about 2 ft. from the tube, and the exposure corresponds to that of about 6 seconds with a lens aperture of f/11. After the negative has been developed and dried, it is cut close and parallel to the image of the capillary tube. The index is then cut into two approximately equal parts, and the two halves are adjusted until one is as nearly as possible a match of the other.

When the two halves are matched the arithmetical mean of any two corresponding readings gives the centre of the index. The three pictures in Fig. 2 elucidate the method. The uppermost shows the positive of the complete index. The left-hand figure shows the two halves matched, the right-hand figure shows an inaccuracy of matching of 2 mm., the mean reading being inaccurate by 1 mm. The figures refer to an index in a tube of about 1 mm. diameter; the index had travelled for two days at the rate of about 1¼ cm. per hour.

Mr. Bell matched the two halves six times in succession. His results were: 28·00, 28·01, 27·995, 28·00, 28·00, 28·00; mean 28·0008 cm.

Mr. F. E. Tinkler, who has frequently assisted Mr. H. G. Bell during the last 12 months, likewise took six readings. His results were: 28·00, 28·025, 27·98, 28·05, 27·96, 28·00; mean 28·0025 cm.

It may be confessed that the negative was a specially good one.

A series of seven pairs of photographs was taken of two indexes in series in a tube of ¼ mm. bore. The negatives were not good, and the probable error of an individual reading, as calculated from the series, came out about twice the probable error of a reading taken with the unaided eye; but the author is convinced that the method specified above, which is satisfactory for a tube of 1 mm. bore, is not adapted for the photographing of an index in a tube of ¼ mm. bore.

§ 4. *Experiments Relating to the Elementary Theory.*

The tube referred to in section 2 and an auxiliary tube of the same diameter, both being originally full of water, were fed with a solution of fluorescein, the speed of flow being of roughly the same value in each case. After a movement of about 15 cm. had taken place the auxiliary tube was disconnected from the flow apparatus and each end sealed with an india-rubber cap. A fiducial mark was made on the auxiliary tube. Then the auxiliary tube was placed alongside the principal tube and its position adjusted, until the ends of the columns in each tube matched, and a reading was taken of the position of the fiducial mark. The flow was allowed to continue in the principal tube, and after some days the columns were again matched and a reading taken of the fiducial mark. The flow in the principal tube was calculated (*a*) by weighing a beaker and contents, as in § 2, and (*b*) from the product of the movement of the extremity of the coloured column, as measured by the auxiliary tube, into the mass of water per linear centimetre of the tube.

In one experiment the end of the column moved at the comparatively rapid rate of about 1⅛ cm. per hour through a distance of about 84 cm. It was obvious by inspection that the end of the moving coloured column was more diffused than the end of the column in the auxiliary tube; nevertheless an attempt at matching was made. The flow calculated from the movement of the coloured column as determined by the auxiliary tube was 1·4 per cent. greater than that calculated from the loss of weight of the beaker.

In a second experiment with the same tube the speed was much slower, being about ⅓ cm. per hour through a range of about 89 cm. The movement as determined by method *b* was

1·04 per cent. greater than that as determined by method *a*. In this experiment there was no characteristic difference between the ends of the two columns.

An experiment was performed with a calibrated tube of ¼ mm. bore; here the weighing method of determining the flow is impracticable, and the flow was measured by placing an index in advance of the continuous column. An auxiliary matching tube of ¼ mm. diameter was employed to measure the movement of the continuous column. The speed was about 1¼ cm. per hour over a range of about 63 cm. The movement of the column as measured by the auxiliary tube was 0·75 per cent. greater than the movement of the index. The reader is reminded that the vertex of the paraboloid moves 100·00 per cent. faster than the index.

Although this section deals particularly with the flow of an indefinitely long column of fluorescein solution, a few words on the determination of the position of a finite index may not be out of place. The author is of the opinion that the best results would be given by the photographic method, if all the details were carefully arranged to suit the tube employed; or else by some equivalent optical device which would enable the two ends to be seen alongside and thus matched directly. At present, however, he would recommend the observer to be content with ordinary eye-readings taken with the assistance of a simple piece of apparatus which is a modification of the strip of black paper mentioned in § 3. It consists of two black metal pieces running along the scale at an adjustable distance apart, each metal piece carrying a vernier, the zero line of which can be brought opposite any desired point near an end of the fluorescein index.

Some experiments, similar to those in § 3, with an index moving at the speed of approximately 4 cm. per hour for about 17 hours in a tube of about 1 mm. diameter, show that with the vernier arrangement the probable error of an individual reading is 0·2 mm. This means that the probable error of a single measurement of the movement is about $\frac{1}{20}$th per cent.

§ 5. *Applications.*

Fig. 3 gives a diagrammatic sketch of an arrangement designed for the introduction of an index into a closed circuit in which there is a circulation of water. T is a capillary tube, R is a small reservoir, A, B, C and D are taps. The ends of the tubes associated with B and C are provided with indiarubber caps. To introduce the index close the taps A and D and run fluorescein solution in at C and out at B. Close the taps B and C, open A and D and allow the flow in the capillary to take place. When the column of fluorescein solution is of the desired length, close the taps A and D, and wash the fluorescein out of the reservoir with water. Now close B and C, and open A and D. *An emphatic warning is given against any trust in glass taps.* If there is a part of the circuit open to the atmosphere, the pressure at the tap A should be determined, and the pressure due to the fluorescein solution, or the pressure due to the water used to wash out the reservoir, should not differ much from the determined value. Otherwise the taps A and D may leak to such an extent as to materially disturb the feeble agency causing the slow circulation. When the circulation is taking place, the ends of the tubes associated with B and C are covered with indiarubber caps, and all the taps are immersed in mercury contained in beakers.

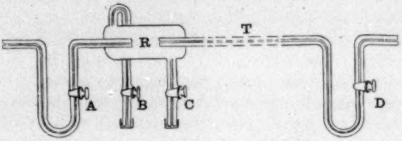

FIG. 3.

A measurement is being made of the coefficient of diffusion of a salt in water by the method of diffusive convection,* and the index (Fig. 2) was obtained with an apparatus designed to apply the method. It is possible that the index may be useful in delicate thermometers and manometers.

All the work recorded in this Paper, except that connected with Fig. 2, was done in a room on the third floor of Birkbeck College. The room has two doors and three windows. It is used by research students, and no attempt has been made to keep a constant temperature. The method of causing a flow along a capillary tube described in § 2 is convenient, but it does not produce a steady flow under the conditions just mentioned, and it is probable that the indexes would be more consistent under superior conditions.

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* "Proc." Phys. Soc., Vol. XVI., Part IV., pp. 230-243, Jan., 1899, or "Phil. Mag.," pp. 453-465, Nov., 1898.

In conclusion, the author desires to express his thanks to Mr. H. G. Bell, to whose skill and patience must be largely attributed the measure of success attained in the experiments described in this Paper.

ABSTRACT.

If a fine capillary tube, originally full of water, is fed with a weak fluorescein solution at a rate so rapid that the diffusion of the fluorescein may be neglected, the vertex of the coloured paraboloid moves at double the mean speed of the liquid. But at slow speeds radial diffusion plays an important part, and the colouring matter travels along the tube approximately as if the liquid moved in a solid column. If, after a short length of fluorescein solution has been introduced into the tube, the supply of solution is replaced by water, a symmetrical coloured column of slowly increasing length is obtained, the centre of which indicates the mean speed of the liquid. The illumination is the light from an arc-lamp which has been transmitted through blue glass, and the capillary tube is placed on a black scale with white graduation marks. It is comparatively easy to determine the mean speed of the liquid to an accuracy of $\frac{1}{5}$th per cent.

DISCUSSION.

Dr. A. Russell gave some illustrations of the extreme slowness of diffusion and remarked that he was sorry the author had not given more mathematics in his Paper.

The President remarked that very few realise the extreme slowness of diffusion. He had once worked out a case of a vertical column of a solution under the action of gravity, and found that the time required to pass from a state of uniform density to the final state of equilibrium was well over a million years.

XVI. *On an Optical Lever of High Power suitable for the Determination of Small Thicknesses and Displacements.*

By E. H. RAYNER.

READ FEBRUARY 24, 1911.

[ABSTRACT.]

THE apparatus shown arose from the necessity of measuring small thicknesses of mica. Something more sensitive than the ordinary micrometer was required. Such a micrometer magnifies about 70 times, 0·01 mm. being about 0·7 on the barrel. The optical levers used magnify satisfactorily about 10,000 times.

The principle of the tilting table was adopted, as used, for instance, in the Ewing extensometer for wires. In this apparatus a " tilting base " of about 20 mm. is used. For the measurement of mica, 1 mm. to 2 mm. was found to give quite satisfactory results.

With a concave mirror at a distance of about 5 metres from the screen and a base of 1 mm. the deflection produced by inserting a thin piece of mica under the third leg magnified the thickness 10,000 times. For calibrating, a piece of mica of approximately the same thickness may be measured with apparatus of the most accurate class and then used as a standard.

The hole and slot arrangement is used for the support of the " twin " feet of the tilting table and a plane or slightly convex surface for the third leg.

These are conically pointed screws which should not bear on their points when it is important to replace the lever accurately in the same position. The third point should not be too sharp, but care must be taken lest its roundness produce an error. For the measurement of paper, &c., a small piece of plain glass, such as may be cut from a microscope slide, may be used to distribute the pressure. The lever may be made a foot long if used to measure over the surface of a large sheet which cannot be cut.

The arrangement would be particularly useful in comparing a number of articles of the same nominal size, such as bicycle balls, if it were very important to have greater equality in a set than could be tested with the ordinary micrometer.

The arrangement has been adapted to measure dilatation coefficients of rods of different materials. A rod of about

150 mm. in length has its ends made flat, and is then supported on a small foot. Its upper end carries a cap on which rests one leg of the lever. Two quartz rods similarly mounted are placed one on either side. The feet and caps of the rods are all similar. They have small cylindrical holes into which project the 90 deg. points of three screws about 12 mm. apart. These vertical screws at the bottom have their points accurately in a horizontal line. They are fixed in a horizontal brass bar, and any movement of this, due to temperature change, has no effect on the movement of the mirror, which is supported by the top of the three rods. The middle one is not quite parallel to the other two, its top being about 1 mm. out of the plane containing the other two rods. This arrangement gives a reading of about 75 mm. per degree for the difference in expansion between brass and quartz of 150 mm. length. The whole is supported from the bottom bar by two vertical rods from an overhead bracket. The rods are immersed in water in a separately supported tank.

Another use for the tilting table is for the measurement of the bending of cantilevers, &c. Three parallel rods project horizontally from the same block of metal, and each supports one of the legs of a tilting table at its free end. If the middle one have a weight added near its free end the deflection is greatly magnified. The instrument shown gave readings of 16·29, 16·29, 16·28 in. deflection for a weight of about 30 grammes using brass rods $\frac{1}{4}$ in. diameter and 200 mm. long, and a tilting table of 4 mm. " base," made of three needle points stuck in a piece of sheet brass.

This affords what may be a very valuable piece of apparatus as an adjunct to a chemical balance. In ordinary weighings the nearest centigramme could be read straight off, and the body then transferred to a balance and its weight readily found to 1/10 mgm. if desired.

The range of a single instrument could be made very large by extending the centre rod by a lighter one for smaller loads. Very large masses could be weighed by using appropriate cantilevers. By this means any weight can be readily determined to an order of accuracy of one part in 1,000 or so.

For calibrating the levers, when such is necessary, a specially designed lever of ratio of about 50 to 1 was shown, attention being particularly paid to obtaining the accurate length of the shorter arm. The longer arm is moved vertically by a micrometer.

In using apparatus of this description it is essential for convenience to reflect the deflected beam of light, by means of a plane vertical mirror, back again to a scale placed near the apparatus.

DISCUSSION.

Dr. S. RUSS asked what was the smallest thickness the optical lever could be used for. He had measured small thicknesses of mica by finding its transmissive power for ionisation.

The PRESIDENT remarked that it was interesting to see to what wide applications the optical lever could be put. The chief difficulty with high power optical levers, especially if the angular displacement was large, was with the shape of the points and the holes into which they fitted.

The AUTHOR, in reply, stated that the smallest thickness he had used the lever for was 3μ. For accurate work the hole, slot and plane mounting should be used, the middle leg resting on the plane.

XVII. *A Demonstration of the Working of the Anschütz Gyro Compass.* *By* G. K. B. ELPHINSTONE.

READ MARCH 10, 1911.

[ABSTRACT.]

THE instrument is really a compass, inasmuch as it points in a definite direction on any part of the earth's surface—that is to say, true North and South.

The apparatus consists of a rapidly rotating wheel enclosed in a casing which is so suspended that the centre of gravity of the suspended system is considerably below the point of suspension, and therefore acted upon by gravity like a pendulum.

With the axle of the gyro in any direction except parallel to the Meridian the gyrostatic action of the rapidly rotating wheel tends to maintain the plane of rotation fixed in space irrespective of the rotation of the earth, and, in consequence, a dipping down of one end of the axle and an upper inclination of the other end takes place.

The force of gravity exercises a couple tending to restore the axle to the horizontal position, and a precession results which turns the axle towards the Meridian, this precession being reversed should the axle once more reach the horizontal position after it has crossed the Meridian.

A natural tendency of a gyrostat suspended in a frame as described above, provided the method of suspension introduced very little friction, would be to swing from side to side of the Meridian for a very long period of time.

On this account a method of damping these swings without the introduction of friction is required. This is obtained by making use of the current of air set in motion by the rapidly revolving wheel. This current of air is led out through a horizontal tube at the lowest part of the gyro casing and tangential to its periphery; the air blast acts horizontally.

When the gyro axle is horizontal—that is to say, when precession in any one direction has ceased—the air blast is symmetrically situated as regards the vertical line through the suspended system, and on this account the air blast may be considered as divided into two blasts equal in speed and volume and distance from the centre line, and under such conditions there is no tendency for the re-action of the still air to cause any movement of the gyro casing other than a slight swinging of the suspended system in a plane coincident with the plane of rotation of the gyro. If, however, the axis of the gyro is inclined from the horizontal position, which is the case whenever the gyro is precessing and therefore not coinciding with the Meridian, then the outlet for the air current is tilted to one side of the centre line, and the reaction caused thereby is in such a direction as to apply a turning moment about the vertical axis of the suspended system, and under the influence of this turning moment a precession of the gyro axle takes place vertically in such a direction as to bring the axis once more horizontal.

In considering the gyro at different points on the earth's surface it is clear that if the two ends of the gyro axle be equal in weight, and if the gyro hangs perfectly freely so that the axle is horizontal when the gyrostat is not running, that under these conditions it is only at the Equator that the axis of the gyro and the axis of the earth can be said to be strictly parallel to one another.

In such a position the axis of the gyro travels on the surface of a cylinder when the earth rotates.

At any other latitude than the Equator the axis of the gyro, being constrained to remain horizontal on the earth's surface, will travel round in space on the surface of a cone, and under these conditions a constant precession of the axle in space must take place, the value of this precession being proportional to $\omega \sin \lambda$, where ω is the angular velocity of the earth and λ is the angle of latitude.

The moment the axle begins to lag behind the Meridian a portion of $\omega \cos \lambda$ causes a tilt, which introduces the gravity couple, but simultaneously the damping couple tends to destroy this by making the axle horizontal. Hence there will be some position of lagging behind the Meridian, for which the damping just maintains the axle at that tilt at which the gravity couple can cause the necessary precession about the vertical. By the action of the earth's rotation the gravity couple is always tending to be increased by an amount which the damping is simultaneously tending to diminish, and thus the couple is kept constant to cause the constant precession $\omega \sin \lambda$.

It is, of course, clear that this precession is only constant for a given latitude. By placing a small weight on one end of the gyro spindle a couple could be introduced to suit any particular latitude, and yet the axle can remain horizontal; but any change of latitude from that for which the weight is adjusted will necessitate a small correction.

If the axle of the gyro is pointing North and South it is clear that any movement of the ship due East or West is merged in the movement due to the earth's rotation, and is negligible in amount.

A movement, however, due to steering a northerly course will produce a couple deflecting the gyro compass westwards, owing to the fact that the axle always tends to keep to its direction in space. Similarly, in the case of a southerly course, the compass is deflected eastwards.

DISCUSSION.

Dr. C. **Chree** commented on the two corrections which had to be applied, one of which depended upon the component of the ship's velocity along the Meridian. He inquired as to the stress in the rotating wheel, and as to how long the material would stand that stress continually. He remarked upon the importance of accurate steering for gun fire, and saw in this direction an important sphere for the gyro compass. He wanted to know if pitching and rolling had any effect on the compass.

Mr. A. **Campbell** asked what power the compass took.

Dr. A. **Russell** asked how far Dr. Anschütz got in making a gyro compass with three degrees of freedom, so that it always pointed parallel to the earth's axis, and remarked that if another such gyro was made so as to keep parallel to the direction of gravity at Greenwich the two together would give both the latitude and longitude of a ship.

Mr. C. W. S. **Crawley** asked how the gyro compass withstood gun fire.

Capt. L. **Chetwynd**, R.N., said he looked to the gyro compass as the way of helping them out of their greatest difficulties. It had come along just at the moment when the difficulties connected with the magnetic compass in modern warships had become almost insuperable. Replying to Mr. Crawley, he said the gyro compass had been very severely tried under gun fire, which had no effect whatever on it.

Mr. **Elphinstone**, in reply, said that as the compass was suspended on gimbals pitching and rolling had no effect. The stress in the rotating wheel, which was made of special Krupp steel, was about 10 tons/in.^{2} Every compass was tested by being run for 700 hours continuously. Several had been in use on and off for three years, and some had run 4,000 hours continuously without mishap. In reply to Mr. Campbell, he stated that the power taken was from 1 to 1·25 amperes in each phase at 110 volts. In reference to a gyro of three degrees of freedom, mentioned by Dr. Russell, he said that the constructional difficulties would be enormous. It would be necessary to keep the centre of gravity absolutely at the intersection of the axes about which the gyro frame rotated while the gyro was running. Gyros with three degrees of freedom had been tried in the German Navy, but given up in favour of the Anschütz, of which about 40 were in use.

XVIII. *Note on an Electrical Trevelyan Rocker.* *By* W. H. Eccles, *A.R.C.S.*, *D.Sc.*

Received February 16, 1911. Read March 10, 1911.

All students of acoustics will remember the curious method of producing sounds that goes by the name of the Trevelyan Rocker experiment. A bar of brass or copper whose cross-section possesses two adjacent salient angles, is heated to some temperature below red heat and laid across a cylinder of lead, with the edges corresponding to the angles just mentioned resting on the lead. If circumstances are favourable, the apparatus yields a loud musical note which persists till the temperature of the copper bar has fallen considerably. The explanation of the experiment is this: When one copper edge touches the lead the heat passing from the copper to the lead causes the latter to expand locally; the expansion is so sudden that the edge is kicked away and the copper bar given a rotary motion which brings the other copper edge down on the lead. This edge now causes expansion and is in its turn repelled. The process may take place hundreds of times a second and usually with sufficient regularity to give a musical note. Lead is the best common metal to use because of its high coefficient of expansion with temperature and its low thermal conductivity, but other metals can be made to work.

It is an obvious elaboration of this experiment to pass a current of electricity from one metal to the other with a view to creating the necessary local heatings and expansions. The experiment was performed by Rollman * in 1850, and independently by Forbes † in 1859, and large masses can be made to rock by means of large currents at low voltages. A discussion of the whole matter by Tanner ‡ leads to the conclusion that all observers of the phenomenon agree that the current is never completely interrupted during the rocking action, one of the alternate contacts being " made " before the other is " broken." It is clear that a considerable current must be driven through the contact points if energy enough is to be supplied to keep a heavy rocker in full motion, but quite small currents can be used for vibrating small masses. Appleyard * describes a large microphone consisting of a horizontal carbon rod laid across two other horizontal carbon rods fixed on a resonance box, which gives a loud sound with currents of about one ampere.

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* Poggendorfs " Annalen," Vol. CV., p. 620.

† " Phil. Mag.," Vol. XVII., p. 358.

‡ " Electrical Review," Aug. 22, 1890.

The following experiments were projected during a search for a method of impressing regular and sudden interruptions on a small current; the photographic records given below were obtained only recently. The sudden making or breaking of the current passing through the inductance of a vibratile circuit affords a very good method of generating feeble electrical oscillations if the discontinuities can be made with great and equal suddenness every time. The Trevelyan Rocker appeared likely to give sharp interruptions and was tried. It did not prove so successful for the purpose as other methods, and was therefore put aside. The present note describes the experiments for the sake of any intrinsic interest they may possess.

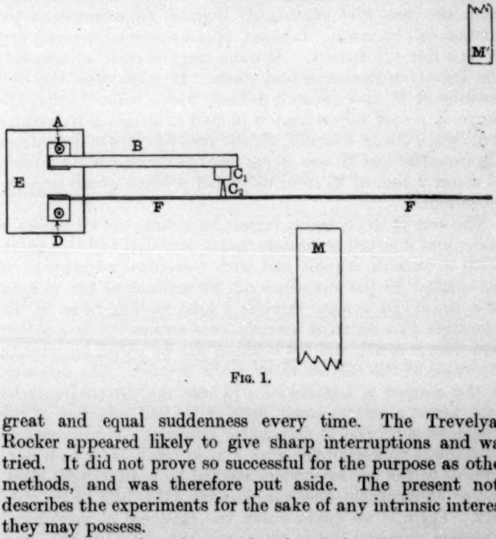

FIG. 1.

A good way of setting up the electrical rocker or vibrator was hit upon by adapting a simple piece of apparatus previously used by the writer in the study of microphonic contacts. This is shown diagrammatically in plan in Fig. 1. Here

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* "Electrical Review," May 14, 1897.

E is an ebonite block carrying a stout brass bar, B, and a thin iron wire, F. These carry the pieces of conductor C_1, C_2, that form the contact. By means of the terminals A and D a current can be sent through the contact. The circuit usually included also a potential divider, a telephone, and a shunted fibre galvanometer. The apparatus is employed in the following manner. The permanent magnet M is pushed towards the iron wire, F, till the contact is just broken, the magnet M′ being meanwhile at a considerable distance. Now M′ in its turn is brought towards the other side of the iron wire till the contact is re-made—an event that produces a sound in the telephone. By arranging that M′ is always a fair distance from the iron wire exceedingly delicate adjustments of the contact can be made. In short, M is a coarse adjustment and M′ is a fine adjustment. At some stage of these adjustments the Trevelyan Rocker action starts. In most cases the best position of M′ is quite well defined, which indicates that the range of proper adjustment is limited to within a fraction of a micron. As an example, on one occasion M was 3 cm. from the iron wire and M′ was 10 cm. from the wire, while a motion of about 2 mm. of M′ from its proper position would stop the vibrations.

The best of the common metals for getting the vibrations is lead; and it is well to arrange that C_1 is a plate and C_2 a point. With a suitable current and with occasional adjustment of the magnet M′ the vibrations can be maintained for an hour at a time. Of course, there is a loud rattling noise in the telephone (the sound is scarcely ever musical when a rather long wire is used), and the loudness can be varied by altering the value of the current through the contact. The direction of the current is immaterial. In size the current must be kept below a certain small limit or the self-induction of the telephone gives rise to destructive sparking. The amount of mechanical motion of the wire F is so small as to be imperceptible to the eye even when assisted by a magnifying glass, and to be inaudible, however close the ear is held to the contact. There is no doubt about the existence of mechanical vibration, however, for in the course of a short time the lead point always becomes flattened by the hammering action at the contact. It is this flattening of the point that makes occasional adjustment of the magnet M′ necessary if the apparatus operates for long. Harder metals than lead have been used, but none give such definite make and break or such regularity as lead. A copper point against lead works very well, steel against steel works irregularly and rather weakly. Pairs of such substances as galena, pyrites, graphite and the

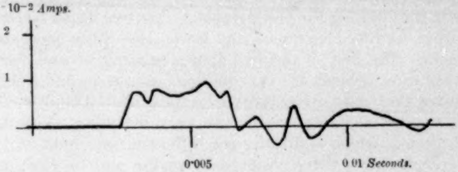

FIG. 2*a*.—LEAD AGAINST LEAD.

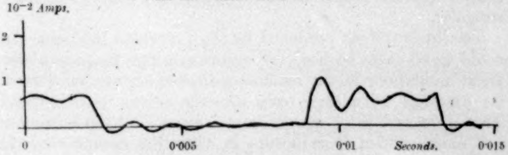

FIG. 2*b*.—LEAD AGAINST LEAD.

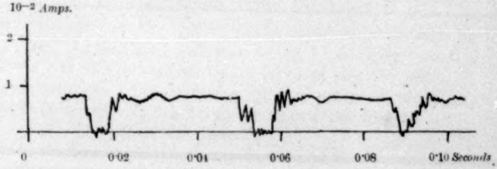

FIG. 2*c*.—STEEL AGAINST STEEL.

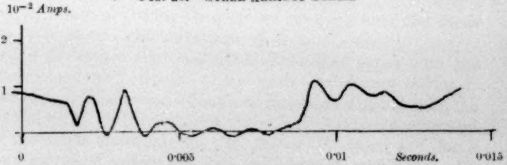

FIG. 2*d*.—STEEL AGAINST STEEL.

metals can be made to vibrate by careful adjustment. The above conclusions were reached by observations with the telephone. In Fig. 2 are given a few tracings of selected photographic records obtained by aid of a fibre galvanometer and a falling plate camera. The fibre was rather too heavy and insufficiently damped for this particular purpose, and therefore allowance must be made for the fibre's own vibrations in interpreting the photographs. The two upper tracings are from lead-lead contacts, the lower ones from steel-steel contacts. The first of the two figures relating to lead shows that the interruptions are very sudden, but the second tracing indicates that great irregularities are possible. The interruptions at steel contacts are usually very irregular. The first steel tracing, which is drawn on a different scale from any of the others, shows three consecutive makes and breaks; the second figure is a typical make and break, and is extremely irregular.

The interruptions produced by the Trevelyan Rocker action could in all cases be used for generating high-frequency electrical oscillations in the manner indicated above, but showed no practical advantage over directly driven interruptions. That is to say, the "make" and "break" of the current is not more sudden than occurs in vibrating contacts of the ordinary type of make and break apparatus.

XIX. *Some Notes on the Tilted Gold Leaf Electrometer, with Suggestions as to the Manipulation of Gold Leaf Suspensions, Suitable Insulators for Electrometer Work, &c. By* G. W. C. KAYE, *D.Sc., B.A., The National Physical Laboratory.*

READ MARCH 10, 1911.

THE tilted electrometer, designed by C. T. R. Wilson * in 1903, has the advantage of a small electrostatic capacity (a few centimetres as compared with about 80 for a Dolezalek electrometer), and offers at will not only a possible high sensitivity but a large range of sensitivities.

This Paper gives an account of a new form of the electrometer, in which the inconveniences of the early model (the extended use of which was never contemplated by Mr. Wilson) have, it is hoped, been removed.

An appendix to the Paper deals with the cutting and mounting of gold leaves, while in a second appendix some electrical insulators are discussed.

The Principle of the Tilted Electrometer.

The principle of the tilted electrometer may briefly be explained. An insulated gold leaf, L (Fig. 1), hanging from C is attracted out of the vertical by an inclined charged plate, P, in its proximity. An alteration in the potential of either, causes L to move. It can be shown that depending on

(1) The tilt of the plate P,
(2) The relative potential of P and L,
(3) The proximity of P and L,

the equilibrium of the leaf may either be stable over the whole of its path, or unstable over part of it.

Fig. 2 illustrates the possibilities that may occur with the electrometer, the potential of the plate being kept constant. In curve I. the deflection of the leaf increases steadily with the potential on it, and throughout there is complete stability and only moderate sensitiveness. In curve III. there are, for a certain range of potentials, two positions of equilibrium for the leaf. The central dotted portion of the S is experimentally unrealisable, and indicative of instability.

By adjusting the conditions, curve II. can be obtained. Here, the deflection is single-valued throughout and although the sensitiveness is high over part of the path, there is complete stability.

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* C. T. R. Wilson, "Proc. Camb. Phil. Soc.," XII., p. 135, 1903.

The critical curve between stability and instability will have a vertical portion indicating infinite sensitiveness; and the tilted electrometer derives its sensitiveness by working as near as may be expedient or desirable to this critical state.*

It is evident that, except for moderate sensitiveness, leaf deflections will not be proportional to leaf potentials over any considerable range. Such proportionality cannot exist at high sensitiveness for any instrument which owes its sensitiveness, as this does, to its ability to work in a condition near instability.

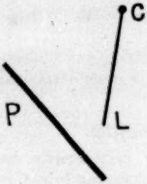

FIG. 1.

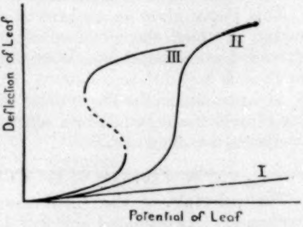

FIG. 2.

Description of the Electrometer.

Fig. 3 gives a general view of the electrometer which is shown in section to scale in Fig. 4. An earth-connected rectangular case, B (Fig. 4), contains within it the insulated plate, P, which can be screwed in or out by rotating the ebonite disc, D. P is connected through the terminal screw T to a source of steady potential (about 200 volts). E is an ebonite sleeve designed as an additional protection for the observer when adjusting D with T "alive."

The gold leaf, L, is supported by means of a wire, which passes through fused silica insulation in the case to the make and break cup, C. A is an earthed tube for shielding purposes. The leaf is observed through the small windows in the case, by a microscope which is provided with a scale in the eye-piece, and has the necessary adjustments for following and focussing the leaf. The whole is mounted on a substantial rigid base which can be tilted by the screw, S (Fig. 3), and carries a binding screw for earthing the case.

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* The sensitiveness of "String Electrometers" is similarly derived. *See* Laby, "Proc. Camb. Phil. Soc.," XV., p. 106, 1909.

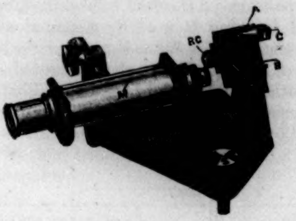

FIG. 3.

The Gold Leaf System and Connections.

The electrical connections are shown in Fig. 5; most of the features are referred to elsewhere.

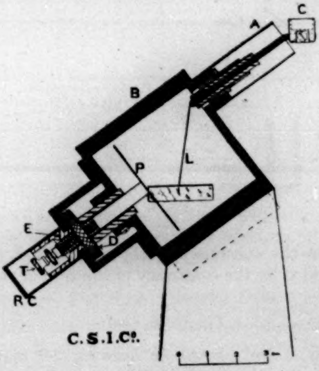

FIG. 4.

Mercury should not be used in the cup, C; it exhibits too much electrification by "splashing," which is frequently evidenced by slight charging up of the gold leaf when the earth connection to the cup is broken. A strong solution of calcium chloride is convenient and free from this defect; its use entails cleaning out the brass cup every few months. If for some reason mercury is preferred, an iron cup should replace the brass one.

The zero-position of the leaf is obtained by earthing the key, K, and lowering it (*e.g.*, by means of a string over a pulley) into the cup, C. When K is raised the leaf is insulated.

The key, K, may also to be connected to a variable known potential which may be used to calibrate the scale of the electrometer. If the sensitiveness is high, only a fraction of a volt will be necessary for the testing potential.

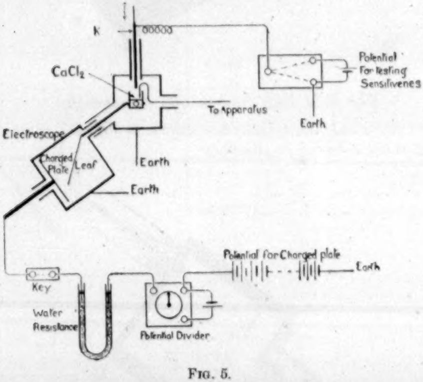
K
$CaCl_2$
Potential For testing Sensitivenes
To Apparatus
Earth
Electroscope
Charged Plate
Leaf
Earth
Earth
Potential for Charged plate
Earth
Key
Water Resistance
Potential Divider

FIG. 5.

A check on the constancy of the potential of the charged plate is provided by the constancy of the zero of the leaf when earthed.

The Gold Leaf.

The length of leaf should lie between 2·9 and 3·4 cms.* Its width may conveniently be 1 mm or less. Methods of cutting gold leaves are discussed in the appendix.

When a new leaf is required, the cup, C, is removed, and the instrument canted over on its side. In this position the gold leaf support and its mounting are vertical and can be withdrawn or inserted without endangering the attached leaf.

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* This, of course, applies only to the present form of the instrument.

The plate, P, is screwed in or out until the end of the leaf *just* clears it. The electrometer is then canted back into the normal position, and the cup, C, is restored.

These relative positions of the gold leaf and charged plate make for the maximum sensitiveness consistent with the safety of the leaf. Should the end of the leaf come in contact with the plate or sides of the case and persist in adhering, the trouble (which is probably due to grease) can be eradicated by taking the electrometer apart, removing the glass windows and the insulation, and boiling the metal case in distilled water with a little soda added.

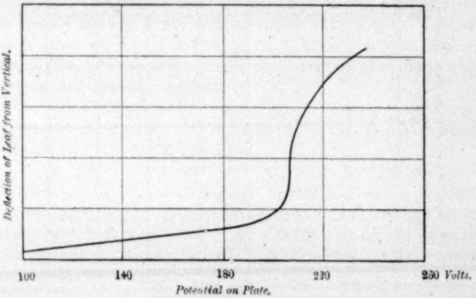

Fig. 6.

The windows are of parallel glass and will ordinarily give no trouble owing to electrification, but windows of conducting glass * can be substituted if desired.

The High Potential Cells.

Good reliable cells, which will maintain the potential of the charged plate, P, of the electrometer constant for some time are a *sine quâ non* for delicate measurements. The constancy of the zero is controlled of course by the steadiness of the potential. Together with all the connections, &c., the cells

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*** Conducting glass owes its origin to Mr. C. E. S. Phillips, and is procurable from Mr. A. C. Cossar, Farringdon-road. Its specific resistance is about 10^8 cm. ohms., *i.e.*, it conducts about 500 times better than ordinary German soda lime glass.**

should be well insulated, and a water-resistance should always be in the circuit between the cells and the electrometer as a precaution against accidental short-circuiting. (*See* Fig. 5.)

A potential divider which will give a volt or so, and act as a fine adjustment on the potential of P may form part of the circuit; it may, of course, be unnecessary.

Some Typical Curves.

Fig. 6 reproduces a typical curve * showing the relation between the potential on the charged plate and the deflection of the leaf from the vertical, the leaf being kept earthed and in metallic connection with the case. It will be seen that for the particular tilt employed (about 30°), the sensitive region is in the neighbourhood of 207 volts.

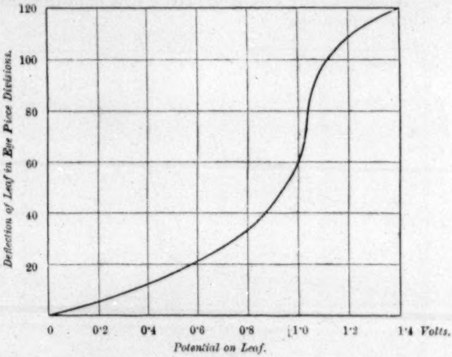

Fig. 7.

Fig. 7 shows a typical relation between the potential on the leaf, and its deflection from the zero. The plate is at 207 volts and the tilt about 30°. It will be seen that in this case the sensitiveness over the central region of the curve is at the rate of some 300 divisions (about 5½ mms.) per volt.†

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* Taken from Mr. Wilson's Paper above.

† It has become customary to give the sensitiveness of electroscopes in eye-piece divisions per volt irrespective of the magnification and the size of the divisions. Sensitiveness should, of course, be expressed as actual distance moved by the leaf per volt.

Some Adjustments.

1. With an average length of leaf and about 200 volts on the plate the sensitive region will be somewhere near the middle of the window when the base of the electrometer is horizontal.

2. For any particular tilt of the electrometer there is a plate potential which will yield a maximum sensitiveness. The less the tilt (or the angle of the plate to the vertical), the greater is the plate potential required and the greater the maximum sensitiveness, until the critical tilt is reached beyond which comes instability over some region ; the extent of this unstable region increases as the tilt diminishes.

3. The greater the sensitiveness the more limited is the range of that sensitiveness and the more susceptible is the instrument to a change in the conditions.

4. To secure the same sensitiveness from a short leaf as from a long requires a higher plate potential and a smaller angle of tilt. A variation in the plate potential of from 160-240 volts may thus be required.

5. As a fine adjustment on the conditions, either the potential, the position of the charged plate, or the tilt may be altered —whichever is most convenient.

I should like to acknowledge my indebtedness to Mr. R. S. Whipple, of the Cambridge Scientific Instrument Co., to whose experience and ability the new design of the electrometer owes a good deal. Mr. C. T. R. Wilson's kindly interest and stimulating suggestions are gratefully recorded.

APPENDIX I.

On the Cutting and Mounting of Gold Leaves.

Most observers have their own way of avoiding the difficulties which attend the cutting of leaves by the novice. There is no point in cutting them narrower than say, ½ mm. for electroscope work ; 1 mm. or 2 mms. is very convenient. Theoretically, a narrow leaf enjoys no advantage over a broad one, but in practice it is found that a narrow leaf is better, being more flexible at the " hinge " * and less perturbed by convection currents.

One good way of cutting gold leaves is to take a book of leaves, isolate one of the metal sheets between the two adjoining grease-free paper leaves, lay them on a piece of cardboard, and cutting with a steady slow stroke through all three sheets by means of a well set razor, separate off a strip of the desired width.

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* For this reason gold leaf is to be preferred to aluminium leaf, which cannot be obtained quite so thin. Weight for weight there is nothing to choose between the two metals in the thinnest leaves in which they are at present procurable.

One other method * of cutting leaves may be mentioned. A pellet of paraffin wax is heated on one side of a clean glass plate which thus becomes coated with a film of wax. While the wax is still liquid, the plate is laid film downwards on an uncreased sheet of gold leaf which is then lifted away with the plate. A second pellet of wax is put on the leaf and a further warming of the plate coats the leaf with wax. When cold, the leaf can be cut into strips of any shape and width by a penknife and ruler.

On warming the plate, a strip can be lifted off on the point of a pin ; the wax resets on the strip and facilitates its manipulation. The strip is immersed in xylol to remove the wax ; it is withdrawn from the liquid on a strip of grease-free paper detached from a book of leaves. The leaf is then freed from the xylol by immersion in absolute alcohol, from which it is withdrawn and allowed to dry on grease-free paper. It dries perfectly flat and is ready for use in a few minutes. Very long, narrow and straight-edged leaves can be prepared in this way.

If desired, the free end of the leaf can, for greater sensitiveness, be expanded into a sort of fantail, but this can scarcely be said to be essential and, unless the wax method of cutting is adopted, requires very considerable manipulative skill. In cutting down a leaf to its length, about 2 mms. should be allowed for the "hinge." When a suitable leaf is ready for mounting, a touch of soft red wax, gum or natural hair grease (which is the resort of bookbinders and other manipulators of gold leaf) is applied to the chisel-ended extremity of the support ; this will be sufficient to pick up and affix the leaf. If a point about 2 mms. from one end is selected for the hinge, the surplus length can be pressed down on the wire to improve the metallic contact. The mounted leaf is then lowered into position in the case.

APPENDIX II.

Electrical Insulators.

Of the available insulators, ebonite, sulphur, amber, sealing wax and fused silica are at present the only ones at all suitable for electrometer work. With all of these, care should be taken to avoid fingering—grease is fatal to insulation: the remedy is good soap and water. In testing insulation it should be remembered that a delicate electroscope may indicate signs of surface electrification for some hours after new insulation has been put in.

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* Beatty, "Phil. Mag." XIV., p. 605, 1907.

Good *ebonite* is difficult to obtain nowadays; it seems to be regarded by most rubber manufacturers as a convenient means of using up rubber refuse unfit for anything else. Some of its defects are occasionally due to the materials used in polishing. Modern ebonite ages with some rapidity in sunlight, and on damp days may, owing to the film of sulphuric acid which forms on its surface, almost play the rôle of a conductor. In a room which gets much sunshine, most modern ebonite usually turns a sort of dirty yellow colour in a few weeks, though some of the ebonite made 10 or 20 years ago will exhibit no signs of deterioration. French chalk can advantageously be applied to restore deteriorated ebonite surfaces. Notwithstanding its defects, ebonite which has had its surface recently renewed is an excellent insulator. Ebonite offers, of course, the great advantage of being easily workable.

Sulphur is convenient in that it can be cast to shape. Needless to say, in this operation the vessels (glass or porcelain) and sulphur used should be clean, and the temperature should be raised no higher than will *just* produce liquefaction. In this limpid condition, it can, for example, be poured into clean warmed glass * tubing if sulphur plugs are required. The tubing can be readily slipped off later by slightly warming the outside. For some hours after solidification sulphur can be turned to size or pared to shape with great ease. There are no better insulators than sulphur, but, after a few months, especially in a room which gets much sunshine, its insulating qualities generally fall off considerably.

Amber is an excellent insulator and is almost always reliable. It can, of course, be obtained in the form of pipe stems which can be mounted in position with sealing wax. The Amberite and Ambroid companies supply amber pressed to convenient shapes and sizes. Amber has the disadvantages of being somewhat brittle and rather expensive.

Sealing Wax is particularly useful in combining the qualities of an insulator and an air-tight cement. The insulating properties depend very much on the quality of the wax. One of the most reliable is " Bank of England." The usual care must be taken to avoid indiscriminate fingering. The insulating ability of the wax will be impaired if in its manipulation it is allowed to catch fire and carbonise, or if a luminous flame is used. As shellac is hygroscopic, sealing wax as an insulator is somewhat susceptible to damp weather.

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* Not metal, unless lined, say, with paper.

Good *Fused Silica* yields place to none in its insulating qualities. Its specific resistance has been determined at the National Physical Laboratory to be greater than 2×10^{14} ohms cms. at 16°C. Fused silica is practically independent of atmospheric humidity, and in the form of rod or tubing is particularly convenient as an insulating material. It is the only high class insulator which is unimpaired by moderate heat; it is however spoilt by very high temperatures.

Fused silica is now very cheap, but unfortunately the modern furnace methods of production cannot be relied upon to yield a product which possesses the insulating properties of the more expensive silica made by the oxyhydrogen flame. This remark applies alike to the clear transparent variety and the air-streaked satin-like kind. In some way the furnace silica seems to be contaminated, possibly by the carbon from the electric furnace. Silica intended for insulation purposes should, of course, be alkali free.

ABSTRACT.

A new model of the C. T. R. Wilson tilted electrometer is described and its mode of use and method of adjustment are gone into. The instrument is provided with an adjustable charged plate, fused silica insulation, a microscope mounted on the same stand as the electroscope case, and has a number of other conveniences. The depth of the case is such as to permit the use of a high power objective.

The electrometer has a small electrostatic capacity, and owes its high sensitiveness to its ability to work in a condition near instability.

An appendix deals with the manipulation of gold leaf suspensions, and reviews the various insulators which are at present available for refined electrometer work.

XX. *The Roots of the Neumann and Bessel Functions.* By

JOHN R. AIREY, *M.A., B.Sc.; late Scholar of St. John's College, Cambridge.*

FIRST RECEIVED DECEMBER 29, 1910. RECEIVED IN ABRIDGED FORM FEBRUARY 10, 1911.

THE first 40 values of x for which the Bessel function of the zeroth order, $J_0(x)$ vanishes, have been calculated by Willson and Peirce,* whilst a general expression for the roots of $J_p(x)=(0)$ has been given by MacMahon.† The second solution of the Bessel differential equation—Neumann's solution—is often referred to as a Neumann function, and is usually denoted by $Y_p(x)$. Only the first four roots of $Y_0(x)=0$ and the first three roots of $Y_1(x)=0$ appear to have been given. These were found by graphic interpolation from Smith's tables ‡ by Kalähne §:—

Roots of $Y_0(x)=0$.	Roots of $Y_1(x)=0$.
0·826(0)	2·11(8)
3·88(5)	5·35(5)
7·01(3)	8·52(1)
10·14(9)	—

The general expression for the calculation of the roots of $Y_p(x)$ and the roots of $\mu J_p(x)+Y_p(x)$, where μ is any real number, positive or negative, can be found by adopting the method employed in determining the roots of $J_p(x)$.

Roots of $\mu J_p(x)+Y_p(x)=0$.

The equation may be written in the form

$$\mu J_p(x)+(\log 2-\gamma)J_p(x)+\frac{\pi}{2}N_p(x)=0. \quad . \quad . \quad . \quad (1)$$

The function ¶ $N_p(x)$ is the same as $-Y_p(x)$ [Schafheitlin] $K_p(x)$ [Graf. u. Gubler] and the Neumann function of Nielsen, $Y_p(x)$.

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* "Bulletin" of the American Mathematical Society, Vol. III., 1896-7.

† "Annals of Mathematics," Vol. IX., 1895.

‡ B. A. Smith, "Messenger of Mathematics," 26, 1897.

§ Kalähne, "Zeitschrift für Mathematik u. Physik," 54, 1907.

¶ Jahnke u. Emde, Funktionentafeln, 1909.

Writing a for $\mu+\log 2-\gamma$, and substituting the semi-convergent series for the functions, equation (1) becomes

$$\left[a\mathrm{P}_p+\frac{\pi}{2}\mathrm{Q}_p\right]\cos\left(x-\frac{2p+1}{4}\pi\right)$$

$$+\left[\frac{\pi}{2}\mathrm{P}_p-a\mathrm{Q}_p\right]\sin\left(x-\frac{2p+1}{4}\pi\right)=0. \quad . \quad (2)$$

Case (*i.*), when a is equal to or less than $\frac{\pi}{2}$.

Divide by $\frac{\pi}{2}$, write b for $\frac{2a}{\pi}$, and put

$$b\mathrm{P}_p+\mathrm{Q}_p=\mathrm{R}\sin\theta_p,$$

$$\mathrm{P}_p-b\mathrm{Q}_p=\mathrm{R}\cos\theta_p.$$

Then $$\tan\theta_p=(b\mathrm{P}_p+\mathrm{Q}_p)/(\mathrm{P}_p-b\mathrm{Q}_p). \quad . \quad (3)$$

and (2) becomes $\sin\left(x-\frac{2p+1}{4}\pi+\theta_p\right)=0.$

$$x=\frac{2p+1}{4}\pi+\theta_p=n\pi. \quad (n=0,\ 1,\ 2\ .\ .\ )$$

$$x=(4n+2p+1)\frac{\pi}{4}-\theta_p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The expression for θ_p in powers of x is found by first calculating the value of $\tan\theta_p$ from (3) and applying Gregory's series to determine the angle from its tangent.

Writing m for $4p^2$ and y for $\frac{1}{8x}$, we get

$$\tan\theta_p=b+(1+b^2)(m-1)y+b(1+b^2)(m-1)^2\,.\,y^2+\ .\ \&c.,$$

and $$\theta_p=\operatorname{arc}\tan b+(m-1)y+\frac{4}{3}(m-1)(m-25)y^3+\dots \quad . \quad (5)$$

The coefficients of the even powers of y and all terms containing b in the coefficients of the odd powers of y vanish. Equation (4) takes the form

$$x=(4n+2p+1)\frac{\pi}{4}-\operatorname{arc}\tan b-\frac{m-1}{8x}-\frac{4}{3}\frac{(m-1)(m-25)}{(8x)^3}\dots$$

or, writing β for $(4n+2p+1)\frac{\pi}{4}-\operatorname{arc}\tan b$, and applying Lagrange's theorem,

$$x=\beta-\frac{m-1}{8\beta}-\frac{4(m-1)(7m-31)}{3(8\beta)^3}$$

$$-\frac{32(m-1)(83m^2-982m+3779)}{15(8\beta)^5}, \quad . \quad (6)$$

where
$$m=4p^2,\ b=\frac{2a}{\pi}=\frac{2(\mu+\log 2-\gamma)}{\pi}.$$

Case (*ii.*) When a is equal to or greater than $\frac{\pi}{2}$, it is readily shown, as above, that x can be expressed in the same form as (6), if

$$b=\frac{\pi}{2a}=\frac{\pi}{2(\mu+\log 2-\gamma)} \text{ and } \beta=(4n+2p+3)\frac{\pi}{4}+\text{arc tan}\, b.$$

These results can be tested when p is half an odd integer.

Roots of $Y_p(x)=0$.

The expression for the roots of this equation is

$$x=\beta-\frac{m-1}{8\beta}-\frac{4(m-1)(7m-31)}{3(8\beta)^3}\ \cdot\ \cdot$$

where
$$\beta=(4n+2p+1)\frac{\pi}{4}-\text{arc tan}\, b$$
$$=(4n+2p+1)\frac{\pi}{4}-0{\cdot}073671.$$

The following tables of the roots of $Y_0(x)$, $Y_1(x)$ and $Y_2(x)$ have been calculated from this formula, except in the case of some of the earlier roots. These were found by calculating the values of the Neumann functions to seven places of decimals in the neighbourhood of the roots from the expressions

$$Y_0(x)=J_0(x)\,.\,\log_e x+\left(\frac{x}{2}\right)^2-\frac{3}{8}\left(\frac{x}{2}\right)^4+\frac{11}{216}\left(\frac{x}{2}\right)^5\ldots\ \&c.,$$

and then graphically representing these results.

Table of *First Forty Roots of* $Y_0(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	0·82601	21	63·54555
2	3·88456	22	66·68705
3	7·01256	23	69·82856
4	10·14876	24	72·97007
5	13·28748	25	76·11159
6	16·42729	26	79·25312
7	19·56766	27	82·39465
8	22·70837	28	85·53619
9	25·84930	29	88·67773
10	28·99037	30	91·81928
11	32·13154	31	94·96082
12	35·27278	32	98·10237
13	38·41409	33	101·24393
14	41·55544	34	104·38548
15	44·69682	35	107·52704
16	47·83823	36	110·66860
17	50·97966	37	113·81016
18	54·12111	38	116·95172
19	57·26258	39	120·09329
20	60·40406	40	123·23486

TABLE *of Roots of* $Y_1(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	2·11827	21	65·10862
2	5·35509	22	68·25048
3	8·52196	23	71·39231
4	11·67528	24	74·53412
5	14·82365	25	77·67592
6	17·96965	26	80·81770
7	21·11434	27	83·95947
8	24·25823	28	87·10123
9	27·40159	29	90·24296
10	30·54459	30	93·38470
11	33·68732	31	96·52642
12	36·82986	32	99·66813
13	39·97226	33	102·80984
14	43·11453	34	105·95154
15	46·25671	35	109·09324
16	49·39882	36	112·23493
17	52·54087	37	115·37661
18	55·68287	38	118·51829
19	58·82482	39	121·65996
20	61·96673	40	124·80163

TABLE *of Roots of* $Y_2(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	3·29690	21	66·65705
2	6·71703	22	69·79990
3	9·94840	23	72·94265
4	13·13551	24	76·08531
5	16·30478	25	79·22788
6	19·46500	26	82·37037
7	22·62002	27	85·51280
8	25·77173	28	88·65517
9	28·92124	29	91·79749
10	32·06920	30	94·93976
11	35·21601	31	98·08199
12	38·36197	32	101·22417
13	41·50726	33	104·36632
14	44·65204	34	107·50844
15	47·79639	35	110·65053
16	50·94040	36	113·79259
17	54·08414	37	116·93462
18	57·22763	38	120·07664
19	60·37093	39	123·21863
20	63·51406	40	126·36060

Roots of $N_p(x)$ [Nielsen's $Y_p(x)$, &c.].

By making $\mu=\gamma-\log 2$, $a=0$ and $b=0$, equation $\mu J_p(x)+Y_p(x)=0$ becomes $N_p(x)=0$.

The expression for the roots of this function is found from (9) and agrees with that given by Prof. MacMahon, viz.

$$x=\beta-\frac{m-1}{8\beta}-\frac{4(m-1)(7m-31)}{3(8\beta)^3}-\ldots,$$

where $\beta=(4n+2p+1)\frac{\pi}{4}=(4n+2p-1)\frac{\pi}{4}+\frac{\pi}{2}.$

Roots of $N_0(x)$ and $N_1(x)$.

The first two roots of $N_0(x)$ and the first root of $N_1(x)$ were found by interpolation from values of these functions in the neighbourhood of the roots. These values were calculated from the expressions

$$N_0(x)=\frac{2}{\pi}\left[\left(\frac{x}{2}\right)^2-(1+\tfrac{1}{2})\left(\frac{x}{2}\right)^4\Big/(2!)^2+(1+\tfrac{1}{2}+\tfrac{1}{3})\left(\frac{x}{2}\right)^6\Big/(3!)^2-\ldots\right.$$
$$\left.-(\log 2-\gamma-\log_e x)\,J_0(x)\right].$$

$$N_1(x)=\frac{2}{\pi}\left[-\frac{x}{2}+\frac{3}{4}\left(\frac{x}{2}\right)^3-\frac{11}{72}\left(\frac{x}{2}\right)^5+\ldots\right.$$
$$\left.-(\log 2-\gamma-\log_e x)\,J_1(x)-\frac{J_0(x)}{x}\right].$$

No. of root.	$N_0(x)$.	$N_1(x)$.
1	0·89358	2·19685
2	3·95769	5·42968
3	7·08605	8·59601
4	10·22234	11·74915
5	13·36110	14·89744
6	16·50092	18·04340
7	19·64131	21·18807
8	22·78203	24·33194
9	25·92296	27·47529
10	29·06403	30·61829

Roots of $\mu J_p(x)+Y_p(x)=0$.

The first two or three roots of the equation, when $\mu=1$, 2, 3 . . . and $p=0$ and $p=1$, have been found graphically by Kalähne from tables of the Bessel and Neumann functions.

Roots of $J_0(x)+Y_0(x)=0$. 0·356(5), 3·34(4)
Roots of $J_1(x)+Y_1(x)=0$. 1·52(2), 4·80(2).

TABLE *of First Ten Roots of* $J_0(x)+Y_0(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	0·35617	6	15·88353
2	3·34532	7	19·02383
3	6·46999	8	22·16449
4	9·60543	9	25·30539
5	12·74386	10	28·44644

TABLE *of First Ten Roots of* $J_1(x)+Y_1(x)=0$.

No. of root.	Root.	No. of root.	Root.
1	1·52101	6	17·42498
2	4·80339	7	20·56985
3	7·97498	8	23·71385
4	11·12971	9	26·85729
5	14·27867	10	30·00035

Roots of $J_0(x)-Y_0(x)=0$, &c.

No. of root.	J_0-Y_0.	J_0-2Y_0.	$10J_0+Y_0$.	$10J_0-Y_0$.
1	1·37616	1·11707	2·55629	2·25096
2	4·46696	4·19582	5·67354	5·36311
3	7·59752	7·32529	8·80753	8·49639
4	10·73440	10·46187	11·94545	11·63407
5	13·87338	13·60073	15·08488	14·77345
6	17·01333	16·74062	18·22505	17·91352
7	20·15378	19·88103	21·36564	21·05408
8	23·29454	23·02177	24·50649	24·19490
9	26·43549	26·16271	27·64750	27·33590
10	29·57658	29·30380	30·78864	30·47702

XXI. *The Vibrations of Circular Plates and their Relation to Bessel Functions.*

By JOHN R. AIREY, *M.A., B.Sc., late Scholar of St. John's College, Cambridge.*

FIRST RECEIVED FEBRUARY 15, 1911. RECEIVED IN FINAL FORM MARCH 7, 1911.

THE vibrations of circular plates were first investigated by Poisson * in a celebrated memoir read before the French Academy of Sciences in 1829. Three cases were considered: (*a*) when the circumference was fixed; (*b*) when the plate was "supported"; (*c*) when the plate was free. The ratios of the radii of the nodal circles to the radius of the plate were calculated when the vibrating plate had no nodal diameter and one or two nodal circles. Kirchhoff † extended Poisson's results for the free plate by calculating six ratios of the radii when the plate vibrated with one, two or three nodal diameters, whilst Schulze ‡ found eight more values of the ratios for a plate with fixed circumference.

The calculation of these ratios required the determination of the roots of equations involving Bessel functions with real and imaginary arguments. These appear to have been found by a "trial and error" method or by interpolation from tables of these functions.

The object of the present Paper is to give a general method of solving these equations—viz., equation (4A) for a circular plate with fixed circumference (Table I.), and equation (9A) for a free circular plate (Tables II. and III.). From the roots so calculated, the radii of the nodal circles and the times of vibration in any given mode are readily found.

(A) *Vibrations of a Circular Plate with Fixed Circumference.*

The displacement of a point on the plate from its position of equilibrium is given by

$$w=A\cos(p\theta)\{J_p(\kappa r)+\lambda J_p(i\kappa r)\}\cos(qt-\epsilon). \quad . \quad . \quad (1)$$

The boundary conditions in this case are $w=0$ and $\frac{dw}{dr}=0$ when $r=a$, or, if the radius of the plate is equal to unity, when $r=1$.

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* "Mémoires de l'Académie royale des Sciences de l'Institut de France," tome VIII., 1829.

† Kirchhoff, "Pogg. Annalen," 1850. Strehlke, "Pogg. Annalen," 1855.

‡ Schulze, "Ann. der Physik," XXIV., 1907.

Hence (1) gives

$$J_p(\kappa a)+\lambda J_p(i\kappa a)=0, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$J_p'(\kappa a)+\lambda J_p'(i\kappa a)=0. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Eliminating λ and writing x for κa, we get

$$\frac{J_p'(x)}{J_p(x)}=\frac{J_p'(ix)}{J_p(ix)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or

$$\frac{J_{p+1}(x)}{J_p(x)}+\frac{I_{p+1}(x)}{I_p(x)}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4\text{A})$$

This becomes, after the substitution of the semi-convergent series for $J_p(x)$, &c.,

$$\tan\left(x-\frac{2p+1}{4}\pi\right)=\frac{I_{p+1}\,.\,P_p+I_p\,.\,Q_{p+1}}{I_{p+1}\,.\,Q_p-I_p\,.\,P_{p+1}}. \quad . \quad . \quad . \quad . \quad (5)$$

$$\text{or} \quad \tan\left(x-\frac{p\pi}{2}-n\pi\right)=\frac{I_p(P_{p+1}-Q_{p+1})-I_{p+1}(P_p+Q_p)}{I_p(P_{p+1}+Q_{p+1})+I_{p+1}(P_p-Q_p)} \quad (5\text{A})$$

$$=-ay-8ay^2-\frac{a}{3}(m^2+2m+93)\,y^3-8a^3y^4$$

$$-\frac{2a}{15}(m^4+6m^3+744m^2-10726m+56055)y^5\ldots,$$

where $a=4p^2-1$, $m=4p^2$ and $y=\frac{1}{8x}$.

Then, by Gregory's series,

$$x=n\pi+\frac{p\pi}{2}-a\,\{y+8y^2+\tfrac{4}{3}(m+23)\,y^3$$

$$+\tfrac{1}{15}(96m^2-18624m+110688)\,y^5\ldots\}$$

$$=\beta-\frac{a}{8x}-\frac{8a}{(8x)^2}-\frac{4a(m+23)}{3(8x)^3}$$

$$-\frac{a}{15}\cdot\frac{(96m^2-18624m+110688)}{(8x)^5}\ldots \quad (6)$$

By Lagrange's theorem, if

$$x=\beta+\frac{p}{x}+\frac{q}{x^2}+\frac{r}{x^3}+\frac{s}{x^4}+\frac{t}{x^5}\ldots,$$

$$x=\beta+\frac{p}{\beta}+\frac{q}{\beta^2}+\frac{r-p^2}{\beta^3}+\frac{s-3pq}{\beta^4}+\frac{t-2q^2-4pr+2p^3}{\beta^5}\ldots.$$

Hence

$$x=\beta-(m-1)\Big[\frac{1}{8\beta}+\frac{8}{(8\beta)^2}+\frac{4(7m+17)}{3(8\beta)^3}+\frac{192(m-1)}{(8\beta)^4}$$
$$+\frac{32(83m^2+218m+2579)}{15(8\beta)^5}\ldots\Big], \quad . \ (7)$$

where

$$\beta=\Big(\frac{2n+p}{2}\Big)\pi.$$

Two roots of equation (4A) were given by Poisson when $p=0$, viz., 3·196 and 6·292, whilst Lord Rayleigh gave the values 3·20 and 6·3. For other values of p, Schulze found the following roots: When $p=1$, 4·612, 7·80, 10·95; and when $p=2$, 5·904 and 9·40.

The roots of equation (4) have been calculated from the expression given in (7), when $p=0$, 1, 2 and 3, some of the earlier roots by interpolation.

TABLE I.

Roots of $\frac{J_{p+1}(x)}{J_p(x)}+\frac{I_{p+1}(x)}{I_p(x)}=0.$

No. of root.	$p=0$.	$p=1$.	$p=2$.	$p=3$.
1	3·1955	4·611	5·906	7·144
2	6·3064	7·799	9·197	10·536
3	9·4395	10·958	12·402	13·795
4	12·5771	14·109	15·579	17·005
5	15·7164	17·256	18·745	20·192
6	18·8565	20·401	21·901	23·366
7	21·9971	23·545	25·055	26·532
8	25·1379	26·689	28·205	29·693
9	28·2790	29·832	31·354	32·849
10	31·4200	32·975	34·502	36·003

By substituting one of these values in (2), the value of λ corresponding to this root can be found—*e.g.*, when the plate is vibrating with one nodal diameter and three nodal circles, $\kappa a=x=10{\cdot}958$, and

$$\lambda=-\frac{J_1(10{\cdot}958)}{J_1(10{\cdot}958i)}$$

or

$$i\lambda=0{\cdot}0000255.\ldots$$

Equation (2) becomes

$$J_1(\kappa r)+0{\cdot}0000255 I_1(\kappa r)=0.$$

The roots of this equation are 3·8312, 7·0024 and 10·958.

Therefore, the ratios of the radii of the nodal circles to that of the plate are

$$\frac{3{\cdot}8312}{10{\cdot}958}=0{\cdot}3496 \text{ and } \frac{7{\cdot}0024}{10{\cdot}958}=0{\cdot}6390,$$

Schulze (" Ann. der Physik.," 1907) gives the values

0·350 and 0·640.

The expression for the frequency of vibration of the plate is

$$\mathrm{N}=\frac{ax^2}{\sqrt{1-\mu^2}}.$$

where a is constant for the same plate (Lord Rayleigh, " Theory of Sound," Vol. I., § 217), μ is Poisson's ratio, and x is a root of equation (4). Since x is independent of μ, N is only affected by a change in the assumed value of μ through the factor $\frac{1}{\sqrt{1-\mu^2}}$.

(B) *Vibrations of a Free Circular Plate.*

The boundary conditions require

$$-\lambda=\frac{p^2(\mu-1)\{x\mathrm{J}_p'(x)-\mathrm{J}_p(x)\}-x^3\mathrm{J}_p'(x)}{p^2(\mu-1)\{ix\mathrm{J}_p'(ix)-\mathrm{J}_p(ix)\}+ix^3\mathrm{J}_p'(ix)} \quad . \quad . \quad (8)$$

and
$$-\lambda=\frac{(\mu-1)\{x\mathrm{J}_p'(x)-p^2\mathrm{J}_p(x)\}-x^2\mathrm{J}\ (x)}{(\mu-1)\{ix\mathrm{J}_p'(x)-p^2\mathrm{J}_p(ix)\}+x^2\mathrm{J}_p(ix)}, \quad . \quad . \quad (9)$$

where x is written for κa.

Eliminating λ, the expressions on the right of (8) and (9) are equal. . . . . (9A)

(i.) When $p=0$, *i.e.*, when there is no nodal diameter, equation (9A) becomes

$$2(1-u)+ix\frac{\mathrm{J}_0(ix)}{\mathrm{J}_0'(ix)}+x\frac{\mathrm{J}_0(x)}{\mathrm{J}_0'(x)}=0. \quad . \quad . \quad . \quad (10)$$

or, using Poisson's value for $\mu(\mu=\frac{1}{4})$

$$\frac{\mathrm{J}_0(x)}{\mathrm{J}_1(x)}=\frac{3}{2x}-\frac{\mathrm{I}_0(x)}{\mathrm{I}_1(x)}. \quad . \quad . \quad . \quad . \quad . \quad (10\text{A})$$

Substituting the semi-convergent series for $\mathrm{J}_0(x)$, $\mathrm{J}_1(x)$, &c., we get

$$\frac{\mathrm{P}_0\cdot\cos\left(x-\frac{\pi}{4}\right)-\mathrm{Q}_0\sin\left(x-\frac{\pi}{4}\right)}{\mathrm{P}_1\cdot\sin\left(x-\frac{\pi}{4}\right)+\mathrm{Q}_1\cos\left(x-\frac{\pi}{4}\right)}=\frac{3}{2x}-\frac{\mathrm{I}_0(x)}{\mathrm{I}_1(x)}=a \quad . \quad (11)$$

or

$$\tan\left(x-\frac{\pi}{4}\right)=\frac{P_0-aQ_1}{Q_0+aP_1}. \quad . \quad . \quad . \quad . \quad (12)$$

Putting $y=\frac{1}{8x}$, $a=12y-\frac{I_0(x)}{I_1(x)}$

$$=-1+8y-24y-192y^3-2016y^4-27648y\ldots$$

This value substituted in (12) gives, after simplification,

$$\tan\left(x-\frac{\pi}{4}\right)=-1-10y-10y^2+320y^3+3650y^4+59840y^5\ldots$$

$$\therefore \quad \tan(x-n\pi)=-5y+20y^2+85y^3+500y^4+21070y^5\ldots.$$

Expressing the angle in terms of the tangent, we find

$$x-n\pi=-5y+20y^2+\frac{380}{3}y^3+20320y^5\ldots$$

or

$$x=n\pi-\frac{5}{8x}+\frac{20}{(8x)^2}+\frac{380}{3(8x)^3}+\frac{20320}{(8x)^5}\ldots.$$

Then, as before, by Lagrange's theorem,

$$x=\beta-\frac{5}{8\beta}+\frac{20}{(8\beta)^2}-\frac{220}{3(8\beta)^3}+\frac{2400}{(8\beta)^4}+\frac{54560}{3(8\beta)^5}\cdots$$

(ii.) When $p=1$—*i.e.*, when the vibrating plate has one nodal diameter, equation (9A) gives

$$\frac{(6+4x^2)J_1-(3x+4x^3)J_0}{(6-4x^2)J_1-3xJ_0}=\frac{(6-4x^2)I_1-(3x-4x^3)I_0}{(6+4x^2)I_1-3xI_0}. \quad (13)$$

This reduces to

$$\tan\left(\frac{\pi}{2}-x+n\pi\right)=9y-84y^2+639y^3-6804y^4+168714y^5\ldots.$$

Hence $x=(2n+1)\frac{\pi}{2}-\frac{9}{8x}+\frac{84}{(8x)^2}-\frac{396}{(8x)^3}-\frac{65261}{(8x)^5}\ldots$

Writing β for $(2n+1)\frac{\pi}{2}$, Lagrange's theorem then gives

$$x=\beta-\frac{9}{8\beta}+\frac{84}{(8\beta)^2}-\frac{1044}{(8\beta)^3}+\frac{18144}{(8\beta)^4}-\frac{385877}{(8\beta)^5}\ldots.$$

(iii.) The general expression for the roots of equation (9A), as far as the term containing $1/(8\beta)^4$ can be obtained from a result of Kirchhoff's, viz.,

$$\tan\left(x-n\pi-\frac{p\pi}{2}\right)=\frac{B/(8x)+C/(8x)^2-D/(8x)^3\ldots}{A+B/(8x)+D/(8x)^3\ldots}\ldots,$$

where $A=\gamma=\frac{4}{3}$, using Poisson's value of $\mu=\frac{1}{4}$,

$$B=\gamma\,(1-4p^2)-8,$$

$$C=\gamma\,(1-4p^2)(9-4p^2)+48(1+4p^2),$$

$$D=-\frac{\gamma}{3}\{(1-4p^2)(9-4p^2)(13-4p^2)\}+8(9+136p^2+80p^4)$$

$$\tan\left(x-n\pi-\frac{p\pi}{2}\right)=-(4p^2+5)\,y+4(16p^2+5)y^2$$
$$-\tfrac{1}{3}\,(64p^6+304p^4+1804p^2-255)y^3\ldots.$$

Writing β for $(2n+p)\frac{\pi}{2}$ and m for $4p^2$, it is easily shown that

$$x=\beta-\frac{m+5}{8x}+\frac{4(4m+5)}{(8x)^2}-\frac{4(m-1)\,(m+95)}{(8x)^3}+\frac{0}{(8x)^4}\ldots$$

$$\therefore\ x=\beta-\frac{m+5}{8\beta}+\frac{4(4m+5)}{(8\beta)^2}-\frac{4(7m^2+154m+55)}{3(8\beta)^3}$$
$$+\frac{96(m+5)\,(4m+5)}{(8\beta)^4}\ldots. \qquad (14)$$

This series is not convergent enough to give the earlier roots of equation (9A). These can be obtained without difficulty from tables of Bessel functions.

Poisson (Mém. Acad., 1829) found the first two roots of equation (9A), when $p=0$, viz., 2·9815 and 6·1936.

Kirchhoff calculated some of the roots of the general equation (9A) by expressing it in the form

$$0=1-\frac{x^4}{A_1}+\frac{x^8}{A_2}-\frac{x^{12}}{A_3}+\ldots$$

and finding the roots by "trial." Only the first two roots, $(\lambda l)^4$, in each case ($p=0$, 1, 2, 3) could be calculated from the table of values of A_1, A_2, &c., given in the Paper. Kirchhoff's roots are readily expressed in the same form as those calculated from (14). Several of these values have been verified and are included in the following table :—

TABLE II.

Roots of Equation (9A).

When $p=0, 1, 2, 3$. $\mu=\frac{1}{4}$ (Poisson's value).

n.	$p=0$.	$p=1$.	$p=2$.	$p=3$.
0	...	...	2·348	3·571
1	2·982	4·518	5·940	7·291
2	6·192	7·729	9·186	10·600
3	9·362	10·903	12·381	13·821
4	12·519	14·024	15·556	17·015
5	15·669	17·218	18·721	20·203
6	18·817	20·368	21·880	23·363
7	21·963	23·516	25·035	26·526
8	25·108	26·663	28·187	29·685
9	28·253	29·809	31·337	32·841

The values of $\lambda(p=0, \mu=\frac{1}{4})$ can be found from (8)

$$\lambda=\frac{J_0'(x)}{iJ_0'(ix)}=-\frac{J_1(x)}{I_1(x)},$$

and x has one of the values in column 2 of Table II. When $x=\kappa a=9{\cdot}362$, for example, $\lambda=-0{\cdot}0001299\ldots$. The radii of the nodal circles are readily found from the roots of the equation

$$J_0(\kappa r)-0{\cdot}0001299-J_0(i\kappa r)=0. \quad . \quad . \quad . \quad (15)$$

The four roots of (15) are 2·40406, 5·5369, 8·3662 and 9·3620. Hence the radius of the plate being taken as unity,

$r_1=0{\cdot}25679$ 0·25679,
$r_2=0{\cdot}59143$ 0·59147,
$r_3=0{\cdot}89365$ 0·89381.

For comparison, Kirchhoff's calculated results* are given in the second column.

The roots of equation (9A) vary with the assumed value of μ, but, with the exception of some of the earlier roots, the variations are comparatively small. The value of the roots for any given value of μ can be obtained without difficulty. For example, if $\mu=\frac{1}{3}$ (Wertheim's value),

$$x=\beta-\frac{3m+13}{3(8\beta)}+\frac{40(3m+5)}{9(8\beta)^2}\ldots.$$

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* Strehlke, "Pogg. Ann.," 1855. Lord Rayleigh, "Theory of Sound," Vol. I.

If this expression be divided by the value of x in (14), the new series of roots can be found with considerable accuracy by multiplying the roots in Table II. by this quotient, viz. :—

$$1+\frac{2}{24\beta^2}-\frac{4\beta(6m-5)-6(m+5)}{(24\beta^2)^2}\cdots$$

TABLE III.
Roots of Equation (9A).
$\mu=\frac{1}{3}$ (Wertheim's value).

n.	$p=0$.	$p=1$.	$p=2$.	$p=3$.
0	...	...	2·292	3·496
1	3·013	4·530	5·936	7·274
2	6·206	7·737	9·188	10·595
3	9·371	10·910	12·386	13·820
4	12·526	14·029	15·559	17·015

The first two roots in each column are those given by Kirchhoff (Crelle, 1850, § 85).

The calculated values of the radii of the nodal circles vary very little for different values of μ. Taking the case where the variation is greatest, viz., when $p=3$ and $n=0$, the change in the value of the radius when μ is changed from $\frac{1}{4}$ to $\frac{1}{3}$ is less than 1 in 500. (Lord Rayleigh, " Theory of Sound," Vol. I., p. 363.)

The change in the calculated value of the frequency of vibration of a " free " plate for a given change in μ is easily found from a consideration of the expression for the frequency, viz. :—

$$N=\frac{ax^2}{\sqrt{1-\mu^2}},$$

a being constant for the same plate and x one of the roots of equation (9A). When the value of μ is changed x also changes and both contribute to the variation in the value of N. The factor $1/\sqrt{1-\mu^2}$ introduces a change of about 2·7 per cent. in the frequency when the value of μ is changed from $\frac{1}{4}$ to $\frac{1}{3}$. Since N varies as the square of the root (x), the variation due to this is easily found from Tables II. and III. For example, when $p=3$, a change in μ from $\frac{1}{4}$ to $\frac{1}{3}$ diminishes the first root by about 2 per cent., and, therefore, the calculated value of the frequency is about 4 per cent. less. In the second mode of vibration, the decrease in the frequency is less than 1 in 200, in the third mode less than 1 in 1,000, and so on.

XXII. *On the Curvature of the Spectrum Lines in a Concave Grating.* *By* ALBERT EAGLE, *B.Sc., A.R.C.S., Imperial College of Science and Technology.*

RECEIVED FEBRUARY 10, 1911.

IT is well known that in the case of spectra obtained with prisms or plane gratings the lines are curved and formulæ for the curvature have been given in both cases. It is obvious that since a plane grating is a particular case of a concave grating the spectrum lines must be curved in the latter case also. The astigmation of a concave grating, combined with any curvature of the spectrum lines, will, it is clear, readily spoil the definition, and it becomes of importance to inquire whether any appreciable resolving power is lost under the conditions of usual practice, and also whether such loss is more detrimental in a new form of mounting described by the author * than in Rowland's mounting.

Lord Rayleigh has recently called attention to the fact that the curvature of the spectrum lines in a concave grating does not appear to have been determined.† I have, therefore, thought it of sufficient interest to publish the following investigation.

Let the rulings of the grating be defined by the intersection of the series of parallel plans $y=ne$, where n is any positive or negative integer, with the sphere

$$x^2+y^2+z^2=2Rx, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

so that the plane $x=0$ is a tangent at the centre of the grating and the rulings are parallel to Oz. Let $P=(a, b, c)$ be a point in the source and $P'=(a', b', c')$ in the image. Let $A=(x, y, z)$ be a point on the surface of the grating. Let P and P′ lie on the cylinder

$$x^2+y^2=Rx, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

described on the radius of curvature of the grating as diameter, and having its axis parallel to the rulings. Let y, z, c, c' be small quantities of the first order compared with R; x will then be a small quantity of the second order.

It is well known that with such gratings as are employed in practice the effect of the fourth order terms is negligible. We will hence omit terms higher than the third order throughout.

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* A. Eagle, "On a New Mounting for a Concave Grating," "Astrophysical Journal," XXXI., p. 120, 1910.

† "Encyclopædia Britannica" (eleventh edition), Vol. VIII., p. 249.

We have

$$AP^2=a^2+b^2-2ax-2by-2cz+y^2+z^2+c^2.$$

Putting $a^2+b^2=\rho^2$, and substituting the value of x from (1)we get

$$AP^2=\rho^2-2by-2cz+\frac{b^2}{\rho^2}(y^2+z^2)+c^2,$$

since $1-a/R=b^2/\rho^2$ by (2).

This may be written

$$AP^2=\left(\rho-\frac{by+cz}{\rho}\right)^2+\frac{b^2z^2-2bcyz+c^2\rho^2}{\rho^2}.$$

Hence

$$AP=\rho-\frac{by+cz}{\rho}+\frac{b^2z^2-2bcyz+c^2\rho^2}{2\rho(\rho^2-by-cz)}$$
$$=\rho-\frac{by+cz}{\rho}+\frac{c^2}{2\rho}+\frac{b^2z^2}{2\rho^3}+\frac{bc^2y}{2\rho^3}-\frac{2bcyz}{2\rho^3}+\frac{b^3yz^2}{2\rho^5},$$

correct to terms of the third order. A similar equation holds for AP′. The optical path PA+AP′ must be stationary along the same ruling, hence to find the point A we differentiate with respect to z and equate to zero, which gives

$$(b^2/\rho^3+b'^2/\rho'^3)z=c/\rho+c'/\rho' \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Substituting this value of z, the third order terms become terms in y, which have to be added to the main term, and, since the total coefficient of y must be equal to $n\lambda/e$ for a spectrum line, we see that b'/ρ', the sine of the angle of diffraction, must vary for different points in the spectrum line, it being assumed that b/ρ is constant for all points in the source.

Two cases only are of sufficient interest to consider. The first is when the grating is mounted in Rowland's manner, and the light is diffracted along the normal. The second is when the diffracted light returns along the line of incidence as in a mounting recently described.*

In the first case we may put $\rho=R\cos i$, $b=\rho\sin i$, $\rho'=R$, and b' a small quantity of the second order. Equation (3) then becomes

$$b^2z/\rho^3=c/\rho+c'/R. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Substituting this value of z in the expression for PA+AP′ we get, since the coefficient of y is constant,

$$b'/R=\rho c'^2/2bR^2-(\rho^2-b^2)c^2/2b\rho^3=(c'^2-c^2)/2R^2\sin i. \quad . \quad (5)$$

This equation represents a series of parallel arcs, the arcs arising from $+c$ and $-c$ overlapping if c be not too great. It must

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* A. Eagle, *loc. cit.*

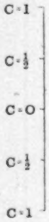

C=1
C=½
C=O
C=½
C=1

Fig. 1.

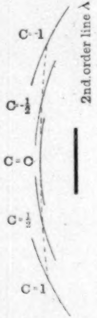

2nd.order line λ = 4250
C-1
C-½
C=O
C=½
C-1

Fig. 2.

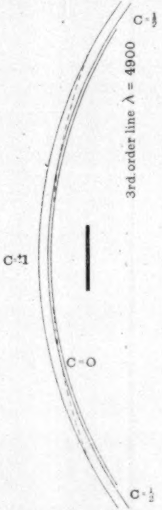

3rd.order line λ = 4900
C-½
C-1
C=O
C-½

Fig. 3.

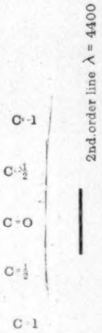

2nd.order line λ = 4400
C-1
C-½
C=O
C=½
C 1

Fig. 4.

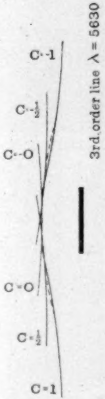

3rd.order line λ = 5630
C-1
C-½
C-O
C=O
C=½
C=1

Fig. 5.

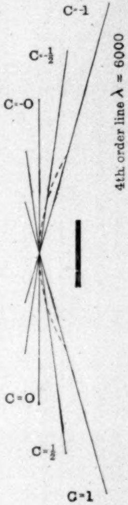

4th order line λ = 6000
C-1
C-½
C-O
C=O
C=½
C=1

Fig. 6.

Diagram showing structure of spectrum lines in a concave grating. The continuous black lines represent light coming from five points at ½ in. intervals on the slit. The dotted line is the line joining the centres of the others and to which the spectrum line reduces in a plane grating. The red end of the spectrum is to the right in all cases. The horizontal scale is 50 times the vertical scale.

Fig. 1. Slit, showing the points to which the different arcs correspond.

Fig. 2. Structure with Rowland's mounting $i=30$ deg.
Fig. 3. ,, ,, ,, $i=60$,,
Fig. 4. ,, new ,, $i=15$,,
Fig. 5. ,, ,, ,, $i=30$,,
Fig. 6. ,, ,, ,, $i=45$,,

The short black lines represent the diffraction width, taken as equal to the distance from the central maxima to the first minima.

To face p. 235.

be borne in mind that the value of z cannot exceed $l/2$, where l is the length of the rulings. Hence, putting $c=0$, we see from (4) that a point at the slit is drawn out into a line of length $b^2 l \mathrm{R}/\rho^3 = l \tan i \sin i$. For a constant value of c, (5) gives an arc of a circle whose radius of curvature $c'^2/2b' = \mathrm{R} \sin i$. The middle point of each arc corresponds to $z=0$, for which $c=\rho c'/\mathrm{R}$ by (4); substituting this value of c in (5) we see that the centres of all the arcs lie on the curve $b' = c'^2 \sin i/2\mathrm{R}$, and is thus an arc of a circle whose radius of curvature is $\mathrm{R}/\sin i$. From the sign of b' it can easily be seen that the *concavity* of the lines as well as of the arc on which their centres lie is towards the red.

If this curvature is not seriously to affect the definition, the path error between rays falling on the centre of the grating and one edge must not exceed $\lambda/4$. Hence the displacement b' must not exceed $\lambda\mathrm{R}/4s$, where $2s$ is the width of the ruled space. Putting $c'=0$ we see from (5) that the displacement at the centre of the line is $-c^2/2\mathrm{R} \sin i$. Hence we must have $c^2 \leq \mathrm{R}^2\lambda \sin i/2s = < \mathrm{R}^2 e \sin^2 i/2ns$, where n is the order of the spectrum. Taking the formula for the astigmatism we see that the centre of the spectrum line will not be illuminated if $2c > l \tan i \sin i$. Applying these conditions to a 6 in., 21 ft. grating, with rulings 3 in. long and 15,000 lines to the inch, we find that the length of the slit illuminated need not be considered in the first order. In the second and third orders the length of slit used should not exceed 0·8 sin i inches (20 sin i mm.) and 0·7 sin i inches (17 sin i mm.) respectively. Figs. 2 and 3 show how the spectrum line is built up for angles of incidence of 30 deg. and 60 deg. in the grating considered.

Consider next the case in which the light is diffracted along the angle of incidence. Let $b'/\rho' = b/\rho + \beta/\mathrm{R}$, where β is a small quantity of the second order. β is then the lateral displacement of any element of the spectrum line. Equation (3) gives us

$$2b^2 z/\rho^2 = c + c'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Substituting this value of z in the third order terms for the expression of the total path length and equating the total coefficient of y to a constant, we get

$$\beta = -\frac{(c^2 + c'^2)\cos 2i + 2cc'}{2\mathrm{R} \sin 2i \cos i}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

For a constant value of c this gives an arc of a circle whose radius of curvature is $c'^2/2\beta = \mathrm{R} \tan 2i \cos i$, with its convex side towards the red if the angle of incidence be less than 45 deg. The length of each of these arcs is obtained by putting $z=\pm l/2$ in (6), which gives $2l \sin^2 i$. To find the locus of the centres of the different arcs we put $z=0$ and, therefore, $c=-c'$ in (7), which then becomes $\beta=c^2 \tan i \sec i/\mathrm{R}$, and is thus an arc whose radius of curvature is $\mathrm{R} \cos^2 i/2 \sin i$, with its concavity towards the red.

The system of arcs given by (7) for the angles of incidence 15 deg., 30 deg. and 45 deg. is shown in Figs. 4, 5 and 6, which are drawn for the same grating as Figs. 2 and 3.

The displacement at the centre of the spectrum line, *i.e.*, at $c'=0$ is $\beta=-c^2 \cot 2i \sec i/2\mathrm{R}$, but it must be borne in mind that light from c will not illuminate the point $c'=0$ if $c>l \sin^2 i$. Considering the same grating as before it is easily found that the definition will never be affected at the centre of the spectrum line in the first order spectrum, while in the second, third and fourth orders the length of slit illuminated should not exceed 1·2, 0·95 and 0·8 in. each multiplied by $\sin 2i \sec^{\frac{1}{2}} 2i$, respectively. These restrictions, it can be seen, are not so severe as in Rowland's mounting.

If the slit of a concave grating be rotated in its own plane, the arcs into which different points of the slit are drawn out into in the spectrum will obviously remain parallel to their original direction, while the arc joining their mid points will be rotated through an angle of the same order of magnitude as the slit, hence a very slight rotation will merely shift the point where this latter arc is tangential to the first set (*i.e.*, the point of best definition) up or down the spectrum line. From this we can see that if the slit were entirely below the plane $z=0$ and the spectrum line entirely above it, the structure of the line could be made exactly the same, by turning the slit a little out of the vertical, as if both slit and spectrum line were symmetrical about the plane $z=0$. Thus no resolution is lost by having the slit entirely below the plane $z=0$, which is necessary in the mounting now considered, but the best definition is secured when the spectrum lines are not quite at right angles to the run of the spectrum. This angle for the grating whose mounting I have described is about $0{\cdot}6 \tan i \sec i$ degrees. Angles of incidence greater than 45 deg. are not used in this mounting, since an angle of incidence of 30 deg. will give the same position in the spectrum as an angle of 90 deg. in Rowland's mounting. Hence this inclination never exceeds 1 degree.

XXIII. *Oscillatory Currents in Coupled Circuits.* *By* G. W. O. Howe, *M.Sc., Whit. Sch.*

Received March 17, 1911. Read March 24, 1911.

If a condenser with a capacity of K farads be charged and then discharged through an inductance of L henries, the resulting current is oscillatory if the resistance of the circuit is less than $2\sqrt{\frac{L}{K}}$. The oscillatory current has a logarithmic decrement of $\frac{R}{2\sim L}$ per period, and if R is small compared with L, a frequency of $\sim = \frac{1}{2\pi\sqrt{KL}}$.

By taking condensers and inductances of suitable values, these oscillatory currents can be shown on the lecture screen by means of the Duddell projection oscillograph. To do this conveniently the ordinary charge and discharge key is replaced by three brushes rubbing on a special commutator which is fixed to the spindle of the oscillograph motor, so that a number of similar wave-trains are superimposed on the screen. As shown in Fig. 1, the commutator consists of a brass cylinder, B, certain parts of which are cut away and replaced by insulating material, I. During a short interval the condenser is connected across a direct-current supply, from which it is then disconnected and discharged through an inductance, in series with which is placed the strip OS of the oscillograph, suitably

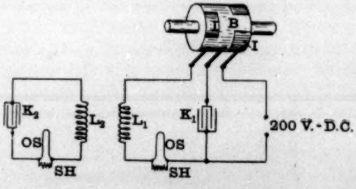


Fig. 1.

shunted. This oscillatory circuit is only broken while the condenser is being charged. In the experiments shown, the capacity consists of four Western Electric Co. condensers, nominally of 5 mfds. connected in parallel, while the inductance is an air-core choking coil of 28 millihenries with a resistance of 0·32 ohm. The oscillations have a frequency of 220 per second. All the curves accompanying this Paper were obtained by placing a photographic film on the tracing desk of the oscillograph and exposing for a few seconds. Figs. 2 and 2A show the curves obtained from single oscillatory circuits. The measured resistance of the circuit was in all cases about half an ohm, while the resistance calculated from the decrement is about 1·5 ohms. (This does not apply to Fig. 2A, which is referred to at the end of the Paper.) The condensers are, therefore, the main cause of damping in the circuits. By

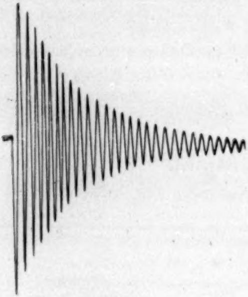


FIG. 2.

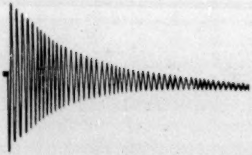


FIG. 2A.

placing a variable resistance in the circuit the damping can be increased until the circuit is made non-oscillatory.

When a circuit contains a spark-gap the conditions are somewhat modified, as the resistance in the gap, and therefore the decrement of the circuit, increases as the current decreases in amplitude.

In the absence of an oscillograph the phenomenon is most easily demonstrated by the mechanical analogy of a gravity or spring-controlled oscillating mass, with a frictional resistance proportional to the speed. If two pendulums are hung from a non-rigid support, such as a stretched cord, they are analogous to two oscillatory circuits with a certain amount of mutual inductance. In the electrical case the E.M.F. induced in the second coil is proportional to the rate of change of the primary current—that is, to what one might call the electrical acceleration. Now the acceleration of a pendulum is a maximum when it is at its maximum amplitude, and that is the moment when, by displacing the point of support, it is exerting the greatest force on the other pendulum.

We shall consider the case in which the two oscillatory circuits are adjusted to have the same natural frequency. The second strip of the double oscillograph is put in the new oscillatory circuit, and the two inductances so placed that they mutually affect each other (*see* Fig. 1). When a discharge passes through the primary circuit, energy will be transferred from the primary to the secondary circuit. If the mutual inductance between the coils is low, the rate of this transfer of energy will be small compared with the rate at which energy is frittered away by the damping in the primary circuit. The secondary current will then never be strong enough to have any marked effect on the primary circuit beyond a slight increase in its damping. In the secondary circuit we shall have a train of the natural frequency, increasing in amplitude up to a certain point and then gradually decreasing. Figs. 16 and 17, Plate I., represent such cases.

If now the coupling between the two circuits be tightened—*i.e.*, if their mutual inductance be increased—the transfer of energy takes place at a greater rate and goes on until the energy is entirely transferred to the secondary circuit, the amplitude of the primary current being reduced to zero. The action then reverses, the role of primary and secondary being interchanged until the energy has been entirely dissipated. This is shown in Figs. 3 to 17, which are all strictly comparable, as the circuits were unchanged except by the introduction of resistance and by altering the distance between the coils. The sensibilities of the two oscillograph strips were adjusted to equality before taking the series of photographs.

Increasing the mutual inductance is represented in the mechanical analogy by decreasing the rigidity of the common support.

It can be shown mathematically that, although the two circuits may be tuned to have exactly the same periodic time, T, when oscillating alone, the oscillatory current in each circuit will now be the resultant of two superimposed harmonic oscillations of different frequencies. If T_1 and T_2 be their periodic times,

$$T_1=\sqrt{T^2+\tau^2} \text{ and } T_2=\sqrt{T^2-\tau^2},$$

where $\tau^2=4\pi^2 M\sqrt{K_1K_2}$. We have already seen that

$$T^2=4\pi^2K_1L_1=4\pi^2K_2L_2.$$

The ratio $M/\sqrt{L_1L_2}$ is generally known as the coupling and designated by k, so that $\tau^2=kT^2$ and $T_1=T\sqrt{1+k}$ and $T_2=T\sqrt{1-k}$.

The superposition of these two harmonic oscillations of different frequencies will result in beats, and if T_B be the periodic time of these beats, it is evident that

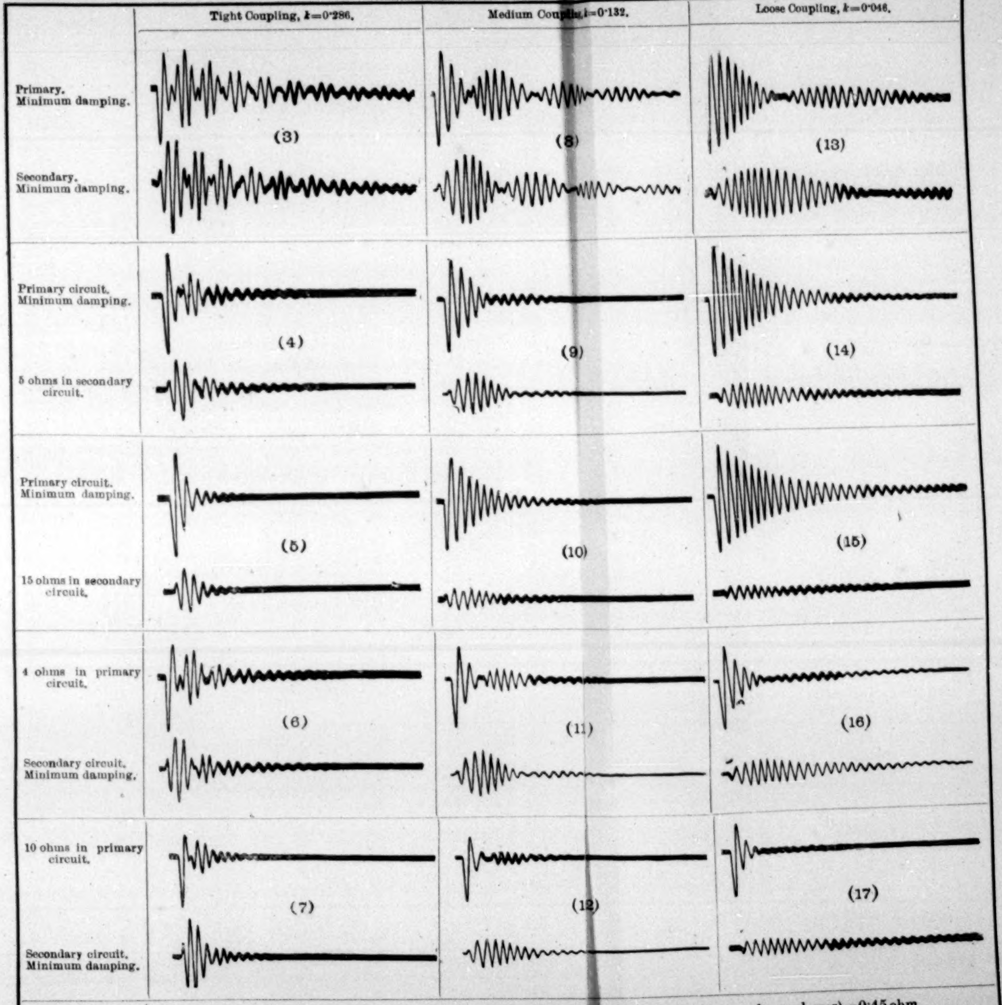
$$T_B=\frac{T_1T_2}{T_1-T_2},$$

and that the number of waves in each beat is

$$\frac{T_B}{T}=\frac{\sqrt{1+k}\,\sqrt{1-k}}{\sqrt{1+k}-\sqrt{1-k}}.$$

A half of this number will be the number of oscillations made by the primary current before the energy has been all transferred to the secondary circuit, which is very important from the point of view of the probability of the spark in a radiotelegraphic sending apparatus being quenched at this moment. In the table the number of complete oscillations per half-beat is given for various values of the coupling. It will be seen that when the coupling is less than a third, which is always the case in practice, the number of complete oscillations per beat is the reciprocal of the coupling. This will be seen to agree very well with the experimental results; for instance, in

PLATE I.



In each circuit, $K=18{\cdot}3$ microfarads. $L=28$ millihenries. R (excluding condenser losses) $=0{\cdot}45$ ohm.
R (from curves) $=1{\cdot}45$.

To face p. 240.

$k=\frac{M}{\sqrt{L_1 L_2}}$.	$\frac{T_s}{T}=\frac{\sqrt{1+k}\ \sqrt{1-k}}{\sqrt{1+k}-\sqrt{1-k}}$.	Number of waves per half beat.
0·05	20	10
0·1	10	5
0·2	4·88	2·4
0·3	3·15	1·6
0·4	2·24	1·1
0·5	1·67	0·84
0·6	1·26	0·63

Fig. 13 the measured coupling was 1/22, and there are about 22 waves in the beat.

Figs. 3 to 7 on Plate I. were obtained with comparatively tight coupling—viz., $k=0{\cdot}286$—the mutual inductance being 0·008 henry, while each coil has a self-inductance of 0·028 henry. In Figs. 8 to 12, k was reduced to 0·132, and in Figs. 13 to 17 it was still further reduced to 0·046. In Figs. 3, 8 and 13 the damping was small, no additional resistance being introduced. In Figs. 4, 9 and 14 a resistance of 5 ohms was inserted in the secondary, and in Figs. 5, 10 and 15 this was increased to 15 ohms. In Figs. 6, 11 and 16 the secondary circuit had the smallest possible resistance, but 4 ohms was inserted in the primary, while in Figs. 7, 12 and 17 this was increased to 10 ohms. Some of these figures could be improved by increasing the sensibility of the instrument, but this would make comparison of the different figures rather difficult. Fig. 18 shows the effect of increasing the sensibility by

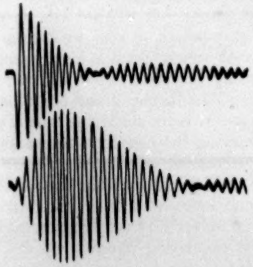


FIG. 18.

partially unshunting the strips, everything else remaining the same as in Fig. 13. It will be noticed at once that the secondary maximum does not coincide with the zero point of the primary oscillation; this is due to the fact that, after a certain point, the secondary damping dissipates energy more rapidly than it is being received from the primary circuit, so that the amplitude of the secondary current decreases in spite of the energy transfer from the primary. So far as the secondary current is concerned it is almost immaterial whether the damping resistance is placed in the primary or in the secondary circuit. The primary current is, however, very different in the two cases.

If the frequency of the oscillations is increased beyond a certain point, the Duddell oscillograph will be unable to follow the rapid variations of the current. The only form of oscillograph which can then be used is that depending on the deflection of the cathode ray in a vacuum tube. In radio-telegraphy the high-frequency oscillations are always investigated indirectly by means of a so-called wave meter, which is simply an oscillatory circuit, the natural frequency of which can be varied over a wide range. It will act as a harmonic analyser, picking out and responding to any oscillation of its own frequency in the circuit with which it is loosely coupled. Figs. 16 and 17 show exactly the conditions in a wave meter loosely coupled with a single oscillating circuit containing a spark-gap.

By placing a hot wire ammeter in the loosely coupled secondary circuit and varying the capacity, resonance curves can be plotted and the decrement calculated. As we can determine the decrement of each circuit from the curves on the screen, we can check the values calculated from the resonance curves and study the errors introduced by coupling too tightly.

By using three oscillating circuits we can represent and study the conditions when a wave meter is loosely coupled with an aerial in which there are two superimposed oscillations.

In the sending apparatus of a radio-telegraph station we are met with this difficulty. If the primary oscillating circuit containing the spark-gap be loosely coupled with the aerial, the electromagnetic waves sent out will have one definite frequency, but the transfer of energy to the aerial will take place so slowly that the major portion of the primary energy will be frittered away in the spark-gap. If, on the other hand, the circuits be coupled tightly, the energy will surge backwards and forwards between the two circuits and powerful electromagnetic beats will be sent through the ether. This will reduce both the efficiency and the possibility of sharp tuning. The obvious way out of the difficulty is to break the primary oscillating circuit at the first moment that all the energy is transferred to the aerial, and thus makes its return to the primary circuit impossible. The possibility of doing this was discovered by Max Wien in 1906. He found that by making the spark-gap very short and thus ensuring that no part of the spark-path was very far removed from a large mass of cold metal, the disruptive strength of the gap is so quickly restored that the circuit is effectually broken if the current in it fall to a small value for a short time. The more efficient the quenching of the spark, the shorter will be the time during which the amplitude of the current must remain small in order to prevent the spark restriking, and therefore the tighter can be the coupling. This is of great advantage, as it lessens the time during which energy is being dissipated in the spark.

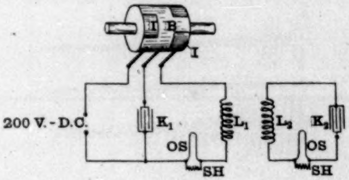


FIG. 19.

To represent this on the oscillograph it is only necessary to break the primary oscillating circuit at the moment when the amplitude of the primary current has fallen to zero. The simplest way of doing this is to rearrange the connections to the commutator as shown in Fig. 19.* The speed of the motor must then be adjusted so that the short interval during which the primary circuit is closed is equal to the time taken for the transfer of energy from the primary to the secondary circuit. The result obtained is shown in Fig. 20. If the quenching of the spark is not effective enough to quench it at the first opportunity it may succeed in so doing the next time the primary

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*** The original intention was to make a special commutator for the purpose; the possibility of simply rearranging the connections to the same commutator was pointed out by Mr. Duddell.**

amplitude falls to zero, as shown in Fig. 21. In Fig. 22 the quenching occurs at the third attempt. As a matter of fact a spark-gap would cause the decrement to be much greater than

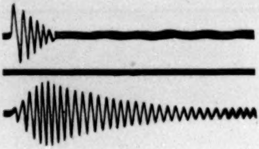

FIG. 20.

that shown in Fig. 22. The spark may be looked upon as a minimum current cut-out which we try to make as quickly-acting as possible.

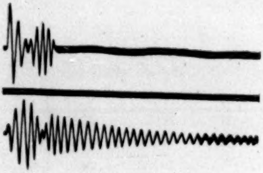

FIG. 21.

All the curves, with the exception of Fig. 2A, were obtained with Western Electric Co. condensers. The decrement in Fig. 2 gives a calculated resistance for the circuit of 1·57 ohms,

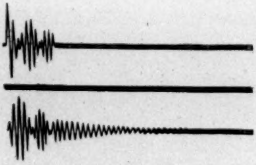

FIG. 22.

whereas the measured resistance as determined by continuous current was only 0·57 ohm. The condensers have thus an equivalent resistance of 1 ohm, and constitute by far the most important source of damping. To see how far the damping could be reduced, these condensers were replaced by far bulkier Varley condensers of 20 mfds. Fig. 2A shows the result obtained. The speed of the oscillograph motor was reduced in order to show more of the wave-train. The decrement is much less and the equivalent resistance of the condensers is only 0·35 ohm. To show the accuracy of this method of finding the losses in condensers the following tables are given showing the determination of the damping from Figs. 2 and 2A.

The writer's thanks are due to his assistant, Mr. F. E. Meade, for his hearty co-operation in taking the photographs which accompany this Paper.

Fig. 2. Western Elec. Co. Condensers. $K = 18{\cdot}3$ mfd. $\sim = 220$.					Fig. 2A. Varley Condensers. $K = 20$ mfd. $\sim = 212$.				
No.	θ	No.	θ	Ratio.	No.	θ	No.	θ	Ratio.
1	79	9	28·5	2·77	1	41·5	13	16·5	2·52
2	69	10	25	2·76	2	38·5	14	15	2·50
3	61	11	22	2·77	3	35·5	15	14	2·54
4	53	12	19·5	2·72	4	33	16	13	2·54
5	47·5	13	17	2·79	5	30·5	17	12	2·54
6	41·5	14	15	2·76	6	28·5	18	11	2·59
7	37	15	13	2·84	7	26·5	19	10·5	2·52
8	32·5	16	12	2·71	8	24	20	9·5	2·53
9	28·5	17	10	2·85	9	22·5	21	9	2·50
10	25	18	9	2·78	10	20·5	22	8	2·56
11	22	19	8	2·75	11	19	23	7·5	2·53
12	19·5	20	7	2·79	12	18	24	7	2·57
Mean value of $\theta_k/\theta_{k+8} = 2{\cdot}774$ Log. decrement per period $= 0{\cdot}127$. $R = 2 \sim L\lambda = 1{\cdot}57$. R (excluding condensers) $= 0{\cdot}57$. Equivalent resistance of condensers $= 1$ ohm.					Mean value of $\theta_k/\theta_{k+12} = 2{\cdot}537$ Log. decrement per period $= 0{\cdot}0775$ $R = 2 \sim L\lambda = 0{\cdot}92$. R (excluding condensers) $= 0{\cdot}57$. Equivalent resistance of condensers $= 0{\cdot}35$ ohm.				

Addendum.—Attention should perhaps be drawn to the ironless choking coils used in the oscillatory circuits. They were designed by Prof. Mather for general laboratory use. They are wound with six-cored cable, the six cores being brought out to a plug-board. The frame work is of wood and fibre and entirely free from metal. The range of inductance is from 7 to 250 millihenries. The power-factor at 50 cycles per second is only 0·03.

XXIV. *High-tension Electrostatic Wattmeters.* By Ernest Wilson.

Received March 1, 1911. Read April 28, 1911.

It is well known that the quadrant electrometer can be used as a wattmeter on alternating-current circuits, and various methods have been devised for connecting it to the mains. The usual method is to impress upon the quadrants an E.M.F. proportional to the current in the main circuit, and to impress upon the needle either the voltage of the mains or a voltage proportional thereto. This is accomplished by passing the main's current through a non-inductive shunt and connecting the quadrants thereto, and using a potential transformer when the voltage of supply has to be subdivided. The drawback to this method is that usually about 1 volt is required across the shunt, and when very large currents have to be dealt with the energy dissipated becomes large. As an alternative, "series" or "current" transformers can be employed, in which case the drop between the terminals of the primary coil can be very much reduced; but when considerable accuracy is required, especially on low power factors, this method is not reliable.

The use of a quadrature transformer whose primary winding is in the main circuit has been successfully developed by Dr. W. E. Sumpner in connection with iron-cored instruments.* It has also been developed in connection with electrostatic wattmeters,† and a diagram of one set of the connections is given in Fig. 1. In this diagram Q is the electrometer, whose moving system is connected to the ends of a non-inductive resistance, R, which is in series with a condenser, K, across the supply mains. The quadrants of the electrometer are connected to the terminals of the secondary circuit of a quadrature transformer, QT.

The action of this instrument depends upon the equation

$$\frac{(2\pi f)^2}{\mathrm{T}}\int_0^{\mathrm{T}} \mathrm{VA}\,dt = \frac{1}{\mathrm{T}}\int_0^{\mathrm{T}}\left(\frac{d\mathrm{V}}{dt}\cdot\frac{d\mathrm{A}}{dt}\right)dt, \quad . \quad . \quad . \quad (1)$$

in which V is the voltage of the supply mains, A the amperes

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* "Journal" Institution Electrical Engineers, 1908, Part 191, Vol. XLI. p. 227.

† British Patent Specifications 26,512, 1905, and 2,707, 1904.

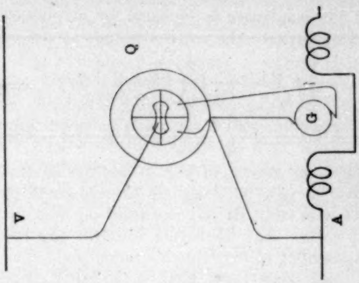

FIG. 3.

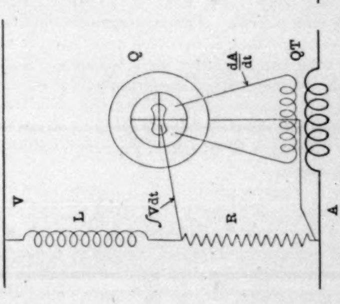
V
L
$\int V dt$
R
Q
$\frac{dA}{dt}$
QT
A

FIG. 2.

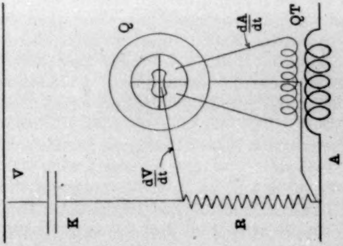
V
K
R
$\frac{dV}{dt}$
Q
$\frac{dA}{dt}$
QT
A

FIG. 1.

in the work circuit, f the frequency of supply and T the periodic time.

In Fig. 2 the condenser is replaced by an inductive resistance L, and in this case the action depends up the equation

$$\frac{1}{T}\int_0^T VA\,dt=\frac{1}{T}\int_0^T\left(\int_0^t V\,dt\right)\frac{dA}{dt}dt, \quad . \quad . \quad . \quad . \quad (2)$$

in which the differential $\frac{dV}{dt}$ is replaced by the integral $\int_0^t V\,dt$.

The arrangement shown in Fig. 1 has the advantage that the condenser K can be designed to withstand very high voltages, but it is only strictly accurate on sine curves, and equation (1) shows that its action involves the frequency f squared. A number of experiments were made with varying wave-forms, and they show that it is fairly reliable with approximately sine curves. The arrangement shown in Fig. 2 has the disadvantage that it involves an inductive resistance, L, capable of withstanding very high voltages. Equation (2) shows that it is independent of variation in wave-form. Each of these types is, of course, afflicted with the error arising from the proportion of the total voltage V to the voltage across the non-inductive resistance R, which on very high voltage systems becomes very small.

Fig. 3 shows in diagrammatic form a high-tension wattmeter whose moving system is directly connected to, and experiences the full voltage of, supply.* The quadrants are connected to a small air-cored generator, G, which rotates in a magnetic field produced by and proportional to the current A in the work circuit. This generator is driven at known speed by a small motor, which can be either direct or alternating current. It was found that when the voltage V impressed upon the moving system is of the order 20,000 volts or more, the quadrants require a considerable voltage in order that accuracy and stability may be secured. In addition, the brushes of the generator have to be carefully adjusted so as to eliminate transformer action. The following are a few particulars of one form of generator which has yielded good results : The armature is of the two-pole type, and is wound with 100 sections, each having 50 turns and an area for magnetic flux of 130 square cms. The commutator has 100 parts. The field coil is built up of copper strip 2 in. deep by $\frac{1}{32}$ in. thick, and has four sections connected to blocks in such wise that they can be used in parallel or series or parallel-series groupings. The number of turns per section is 10. The speed of rotation is 3,600 revs. per min., and is registered by a Hartmann & Braun speed counter, capable of detecting a variation in speed of 0·3 per cent. The curve of voltage between the generator brushes was determined for various positions of a rotating contact-maker fixed to the shaft of the alternator and compared with the curve of current in the field coil. If the generator is to be reliable, these curves should be identical. The curve shown in Fig. 4 and the observation points thereon can be taken as an example of the agreement obtained. The agreement is as close as can be demonstrated by the method of test.

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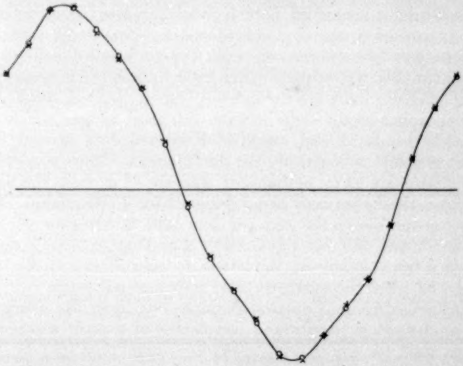
* British Patent Specifications 5,582, 1904, and 27,201, 1904.



O Denotes Volts. **X** Denotes Amperes. Frequency, 4. Speed of Generator, 3,000.

FIG. 4.

The generator verifies the theory underlying its action. In the case before us the armature rotates in a short solenoid having a length of 4¾ in.—*i.e.*, there is a distance between the upper and lower sections of ¾ in. Under these conditions the armature which has a diameter of 4·5 in. rotates in a field which has not uniform strength, but is denser at the centre of the solenoid than near its ends. A field coil has been constructed having its conductors wound on the surface of a cylinder concentric with the armature, and fulfilling the condition that the ampere-turns per unit length of an axis normal to the plane of the windings are constant. The field inside this winding is constant in strength and has the value $\frac{8\pi^2 a^2 nx}{3}$,* where the total turns are $2na$, n being the turns per unit length of the axis, and x the current. This theoretically perfect method of winding is difficult to accomplish when currents of 200 amperes have to be carried, as was the case in the generator first alluded to. It would appear that strip copper $\frac{1}{32}$ in. thick gives at ordinary frequencies an unobservable error due to eddy currents.

As regards the electrometer, it may be mentioned that in its first form† it resembled Lord Kelvin's quadrant electrometer. The suspension was made of phosphor-bronze strip, and the vanes, two in number, were each 10¾ in. long by about 4 in. broad at their widest part. In a later form of the instrument‡ the quadrant pairs are the moving system, and are suspended by phosphor-bronze strip between two fixed sectors, which are heavily insulated and capable of withstanding several fold the normal voltage of the instrument. The suspended quadrants are 9½ in. in diameter, and are 1¾ in. from the fixed sectors, the instrument being designed for 15,000 volts. The instrument obeys the straight line law, is accurate on low power-factor, and has a sensibility such that on a scale 50 in. distant from the mirror, the kilowatts per millimetre deflection are 1·82 when the generator field coils are four series.

A further piece of apparatus, depending upon the charging of condensers in parallel and the placing of them in series, has been used for multiplying a small voltage produced by the passage of the main's current through a low resistance shunt.§

ABSTRACT.

When using the Electrometer as a wattmeter it is necessary (in order to secure accuracy) that the voltage impressed upon the quadrants shall not be less than a certain minimum depending upon the voltage to be impressed upon the moving system. When the latter voltage is of the order 10,000, the quadrants require a voltage larger than can economically be provided by a shunt. One is led, therefore, to consider intensifying devices.

The "series" or "current" transformer, whose secondary winding is closed on a non-inductive resistance, can be used to give

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* Maxwell's "Electricity and Magnetism," Vol. II., §675, Edition 1873.

† British Patent Specification 2,707, 1904.

‡ British Patent Specification 27,201, 1904.

§ *See* British Patent Specification 2,850 of 1905.

fairly good results, but it is not accurate at all frequencies and is dependent upon wave form.

The author's quadrature transformer is a very simple piece of apparatus which can be relied upon to give for electrostatic wattmeters an electromotive force which is strictly the differential of the current in the primary winding. When so used it is necessary, for accuracy, at all frequencies and on all wave forms, that the integral of the mains voltage shall be impressed upon the moving system, although for sine curves only the differential need be impressed instead of the integral.

The best device to impress on the quadrants a suitable voltage in phase with the currents is a generator with an air-cored magnetic circuit as described in the Paper. The mains current is passed through the field coil of the generator and produces a magnetic field proportional to the current; the armature is driven at known speed in this field, and is provided with a commutator and brushes. The brushes when set accurately have a voltage between them proportional to, and having the same wave form as, the mains current.

Another device depending upon the charging of condensers in parallel and the placing of them in series has also been used for multiplying a small voltage produced by the passage of the mains current through a low-resistance shunt.

DISCUSSION.

Dr. **Sumpner** pointed out that the reason why a fairly high voltage was needed on the quadrants together with a strong control on the needle when a very high voltage was applied to the latter, was because of the action between the needle and the case; unless the case was quite symmetrical about the axis of the needle.

Dr. **Russell** inquired whether the author was troubled about brush discharges at the edges of the needle.

Mr. E. H. **Rayner** remarked that he had been using a quadrant electrometer for measuring the energy losses in insulating materials with a sensitiveness 1,000,000 times as great as Prof. Wilson's, giving 0·5 mm. scale deflection per microwatt. The use of a high inductance would introduce errors with low power factors owing to its resistance. The method of working the needle at half the main voltage, having the effect of eliminating the correction for the current resistance, has such advantages that it is in many cases the natural one to use. If a dividing resistance is employed, it will be found that when high voltages and resistances are used the capacity and current of the needle will be comparable with that in the resistance, and at low power factors the error may be serious. The best way to avoid it is to connect the needle to the middle paint of the high-voltage transformer, when such is used; the resistance of its winding is much less than that of any practicable dividing resistance. If a high-voltage transformer is not used for obtaining the potential, a potential divider may be made by connecting two similar switchboard voltage transformers in series across the mains, and connecting the needle to the middle point. There is another error when using a high resistance of the order of 50,000 ohms in the "current" circuit when measuring a watt or less. This is caused in a similar manner to the above. The capacity current between quadrants and needle, in the case of one set of quadrants (assuming one pole of the supply is earthed), has to traverse the current resistance. This causes a deflection on open circuit which is proportional to the resistance used. Like the previous error it may become quite important at low power factors.

Mr. C. C. **Paterson** drew attention to the serious limit of accuracy imposed by not being able to keep the speed constant.

Mr. W. **Duddell** asked if the voltage could not be reduced by means of another quadrature transformer in series with a high resistance, instead of being applied direct to the needle.

Prof. C. H. **Lees** drew attention to the advisability of reducing the voltage by some means before applying it to the needle.

The **Author**, in reply, stated that Dr. Sumpner's remarks were very interesting, as they bore out the statement in the Paper about the minimum voltage to be impressed upon the quadrants. In the case of the instrument exhibited, the *moving* system was at *low* potential relatively to the case. This reversal of the ordinary arrangement helped matters in the above connection, and gave greater accessibility to the moving parts. Dr. Russell raised the question of brush discharge. The author found that brush discharge gave rise to unsteadiness. It could be discovered by observing the instrument in the dark with the high voltage impressed, and where found the radius of curvature could be then increased and thus remove the discharge. The author was glad to hear from Mr. Rayner that he had an electrostatic wattmeter of such great sensibility. The suspension used in the case of the instrument exhibited was of very stout phosphor bronze. If necessary the sensibility could be greatly increased.

When charging the needle, by aid of a non-inductive ratio, at half the voltage of the mains the author could very well believe that when the latter was of the order of 10,000 the capacity current would be large compared with the current in the reducing ratio due to its resistance. This would then give rise to inaccuracy. In Figs. 1 and 2 the voltage on the needle was about 100 when the voltage of the mains was 10,000. It was no doubt a very difficult thing to build an inductive resistance, L, with an air core whose CR component was small enough to render correction negligible. Replying to Mr. Paterson, he wished to say that the speed need not be regulated to any exact figure. All that was necessary was to be able to regulate it so that it came within the range of the speed meter, and then to apply a correction if necessary. Mr. Duddell suggested *two* quadrature transformers—one for the needle in series with a high non-inductive resistance, and one for the quadrants. This would only give accuracy on sine curves. Non-inductive resistances for 10,000 volts or more were very expensive, and the author had avoided their necessity. Potential transformers as a means for reducing voltage were not strictly reliable when great accuracy was desired.

XXV. *Previous Magnetic History as Affected by Temperature.*

By PROF. E. WILSON *and* L. C. BUDD.

RECEIVED MARCH 8, 1911.

IT is well known that if a piece of iron be subjected to a considerable magnetising (previous history) force, and then be tested for permeability corresponding to a lower force, the permeability so obtained may differ widely from the permeability which would have been obtained had the material been previously demagnetised. The effects of previous magnetic history upon permeability and the dissipation of energy by hysteresis have already been studied* at ordinary atmospheric temperature. The object of the present Paper is to examine the effect of variation of temperature upon the influence of large previous history. The material used has been in the form of rings built up from stampings of Stalloy, which is an alloy of iron containing about 3 per cent. of silicon. The stampings are about 0·42 mm. thick, and have internal and external diameters of 32 mms. and 45 mms. Ring 1 was used for high temperatures, and had its windings insulated by hand with sheet asbestos. Ring 2 was used for low temperature, and had its windings insulated with silk. The following are particulars of the rings :—

—	Area of core in sq. cms.	Primary turns.	Secondary winding.	
			Turns.	Resistance in ohms at 7°C.
Ring 1	1·88	30	42	0·26
Ring 2	4·16	47	72	1·66

The method of test employs a ballistic galvanometer, and the temperatures are based upon a measurement of resistance of the secondary windings, care being taken that the magnetising force thus employed did not exceed the smallest reversal force used in the experiment. At atmospheric temperature and the temperature of liquid air the procedure was to determine the ordinary **BH** curve after the material had been carefully demagnetised. This was done by gradually reducing the primary current given by an alternator at 50 frequency, which operation took several minutes to perform. When dealing with high temperatures the ring was placed in a furnace heated by gas, and as the temperature was not absolutely steady in any one experiment it was necessary to apply the previous history force immediately after taking the observation on the demagnetised ring, and then to demagnetise again for the next point, and so on. In all the experiments a previous history force of about 20 C.G.S. units has been employed.

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* *See* "Journal" Institution of Electrical Engineers, Vol. XXXIV., Part 170, p. 55, and Roy. Soc. "Proc.," A., Vol. LXXXIII., p. 1.

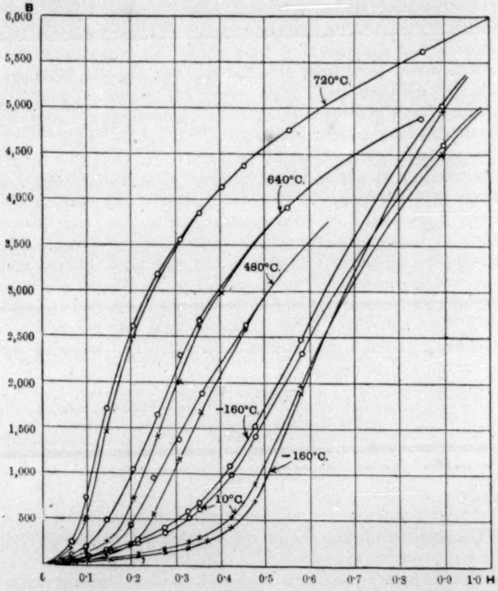

Fig. 1.

Fig. 1 gives a set of curves taken at various temperatures with and without previous history. In every case the upper curve was given by the demagnetised material, and the difference between it and the lower one is due to previous history. It will be seen that in liquid air the effect is most marked, and that it becomes smaller as the temperature increases. The material becomes non-magnetic at about 850°C., and regains its magnetic quality on cooling at 830°C. It seems safe to conclude that just before the temperature at which it becomes non-magnetic is reached the previous history effect has either vanished or become very small indeed.

In Fig. 2 the ratio of the magnetic inductions $\mathbf{B} : \mathbf{B}_1$ for a given value of the reversal force $\mathbf{H}$, with and without previous history, is given. As received from the makers the material of ring 1 gives, at atmospheric temperature, curve 1. Atmospheric temperature curve 2 was obtained after heating to 670°C. and allowing the material gradually to cool. It will be seen that previous history effect is considerably greater after the specimen has been heated, and the other curves also confirm this, the increase being due primarily to a diminution of the permeability after application of previous history. With

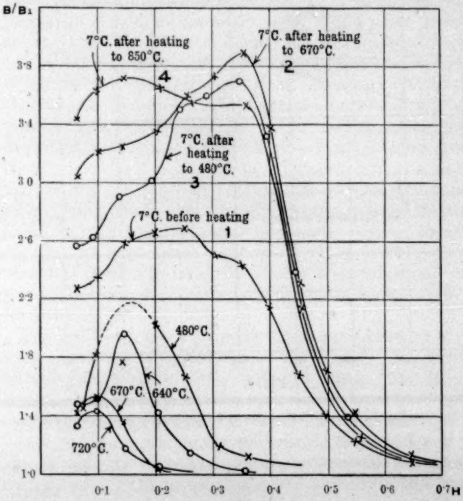

Fig. 2.

regard to the cooling experiment and ring 2, the curves in Fig. 3 show that after the material has been cooled in liquid air, the previous history effect at atmospheric temperature is greater than when first received, the increase being due to increased permeability when in the demagnetised condition. In spite of this, the effect of previous history is greater at the temperature of liquid air than at atmospheric temperature, and it is less at high temperatures.

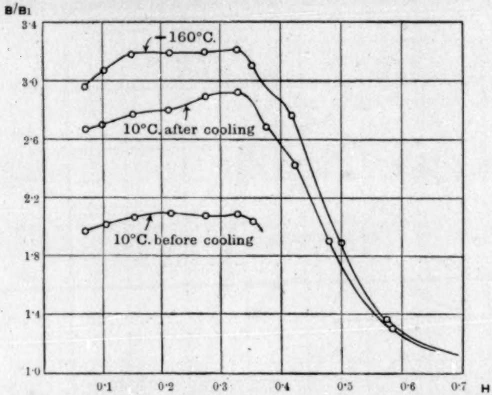

Fig. 3.

The conclusion is that the influence of large previous magnetic history in a 3 per cent. silicon alloy of iron is seriously affected by variation of temperature, being largest at the low temperatures, and ultimately becoming very small, if not vanishing, at the point near which the material becomes non-magnetic. In addition, both heating and cooling have permanent effects upon the magnetic quality of this alloy. Previous heat treatment has also to be considered.

XXVI. *Notes on the Behaviour of Incandescent Lime Kathodes.*

By R. S. WILLOWS, *M.A., D.Sc., and* T. PICTON, *M.A., B.Sc.*

RECEIVED MARCH 13, 1911.

SINCE Wehnelt's discovery that oxides of the alkaline earths emit a large number of negative ions when heated, these substances have been largely used as the kathodes of vacuum tubes. They confer the great advantage that a discharge may be passed by a P.D. as low as 30 volts. Such tubes have been shown before the Physical Society by Mr. Campbell Swinton. In one form or other they have been suggested as oscillation valves for the rectification of small oscillatory currents. As their erratic behaviour at starting frequently results in serious inconvenience, and as they are said to lose their efficiency with continued use, it was thought that a more detailed study of these points might be of interest. It was hoped, also, to throw light on the origin of the negative ions. With respect to the latter point, since the strongly electro-positive metals are found to emit negative ions freely under various conditions, the most obvious source is the metal of the oxide used. This view has been negatived by Horton's experiments,* which show that the current from calcium metal is much less than from the oxide.

The substance used, generally lime, was placed on a metal strip, platinum or nickel, either by heating the nitrate or smearing on a paste of the oxide in water. The strip was heated electrically, in a tube such as has been later used by Garrett† for experiments on aluminium phosphate; it was joined to the negative pole of a suitable battery, which drove the negative ions on to a metal plate connected with a galvanometer, the other terminal of which was joined to the positive pole of the battery. The source of the lime appeared to be without influence on the results. The temperature, when necessary, was obtained by means of a thermo-junction fused to the middle of the metal strip.

The experiments have extended over three years; they have been conducted at pressures of (1) 0·002 mm.; (2) pressures greater than 0·5 mm.; (3) in flames. The results are different according as the voltage used is greater or less than the saturation voltage when such occurs.

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* Horton, "Phil. Trans.," Sec. A, 207, p. 149, 1907.

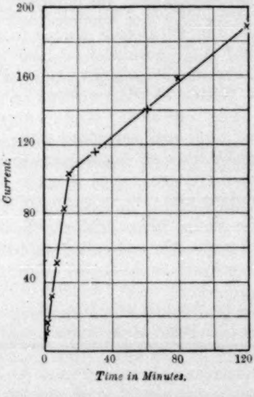
† Garrett, "Phil. Mag.," October, 1910.

EXPERIMENTS AT REDUCED PRESSURE.

Irregularities in Starting the Discharge.—It is often difficult to get much current to pass when the apparatus is first set up; this is especially the case with platinum foil as the support, when using nickel the difficulty was not often met with. A much higher temperature or voltage may be used to start the discharge, after which much lower values of either will give as great a current as is necessary. This procedure often results, unfortunately, in the fusion of the strip. A much safer way is to pass the discharge from an induction coil for a few seconds. The current may be increased by more than a hundred-fold by this means.

It is usual to regard the emission of the ions as being purely a temperature effect. In numerous instances other causes than temperature greatly influence the current. A typical instance may be given. A tube which had been started as above carried current for several hours during the first day. The following morning, when the same conditions as to temperature, pressure and voltage were established, no current passed for several minutes, when suddenly the galvanometer deflection went up to 100 divisions and oscillated violently, finally coming back to zero for some minutes. This behaviour was repeated several times during the first 20 minutes, after which the deflection gradually rose from zero and became steady in the neighbourhood of 100 divisions. During this stage, and frequently at later stages, the current is very sensitive to mechanical disturbance. A slight vibration suffices to reduce the current to zero, or to send it from zero to its full value. Similar behaviour has been noticed with the same strip, each day the tube was used, over a period of several months. We have been unable to determine the conditions necessary for sensitiveness to vibration.

Two other observations which may have some connection with one another may be mentioned. At the higher pressures the gas gradually disappears from the tube as the current is continually passed. At these pressures, 1 mm. or greater, if the lime be first heated before the battery is put on at starting, a current passes which is some hundreds of times the steady value; this rapidly decreases. The same occurs if, after heating for some hours, the battery is cut off but the heating continued. On again applying a P.D. a greatly increased current is observed, as if, in the interval, ions had accumulated in the line only to be driven out when the battery is put on. A curve showing the course of this accumulation is shown in the Figure. After getting the current to its steady value, the battery was cut off for some minutes, the heating being continued. The battery was then re-applied and the galvanometer read as quickly as possible. The current was passed until it again became steady, when the battery was again cut off, to be re-applied at a longer interval. The abscissæ represent the times the ions were allowed to accumulate, the ordinates the initial currents. These two effects have not been noticed at pressures as low as 0·002 mm., nor with voltages less than the saturation voltage.



Alteration of Activity with Time.

Conditions of Experiments.—Lime spread on platinum strip, pressure 0·002 mm. and upwards, voltage 36, which is greater than the saturation voltage, temperature 1,100°C.

Under these conditions, neglecting the initial irregularities, the current gradually increased for two hours, after which it remained steady for the further five or six hours it was heated. The apparatus stood without heating over night. The initial current on the second day was rather higher than on the first, and finished also at a higher value. During the night there was always a loss of activity. This behaviour was repeated for some months, until the platinum fused. The final activity was considerably greater than that at the beginning. One record out of many may be briefly given.

February 24th.—Initial current=50; after six hours heating current=130.

February 25th.—Initial current=60; after six hours heating current=160.

March 10th.—After heating for six hours current=370.

May 18th.—Current=460.

On the last date air got into the apparatus. After this had been pumped out the current was only 32 divisions, but gradually rose again, and apparently started to run through the same cycle of changes. Admission of fresh gas, whether air, hydrogen or carbon dioxide, always produced this sudden fall in current, no matter at what voltage or pressure the observations were being conducted. The recovery was more rapid with hydrogen than with the other gases, especially if it were allowed to stand for some hours in the apparatus. So far, then, from becoming fatigued with time the lime actually increased in activity nine-fold.

Observations with a Nickel Strip.—The results with a nickel strip were rather different. On account of the frequent fusion of the nickel the experiments could not be extended over such long periods.

It was much easier to obtain a visible discharge with nickel than with platinum. A P.D. of 36 volts usually caused a strong visible discharge to pass after a few minutes heating. Under these circumstances the current rose, generally, to a maximum after two hours and then decreased. Upon restarting next day the current was usually, but not always, much greater than at the *close* of the previous day's heating; it rose to a higher maximum and then decreased as before. This behaviour was repeated daily, until the final activity was sometimes 200-fold greater than at the start.

When we were fortunate enough to keep a nickel strip on for more than a week, the gas pressure remained very steady for some days and finally decreased, as in an ordinary vacuum tube.* With platinum there was a steady evolution of gas over three months. A careful spectroscopic examination of the gas in the tube might throw light on the production of the ions; the requisite apparatus was not, however, at our disposal. To test roughly whether the gas in the tube was being changed, an auxiliary vacuum tube was sealed on, and the voltage at the terminals of this was tested with a small coil at intervals. A small rise was observed, but this might be due to the electrodes altering on account of gas escaping from the metal.

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* Willows, "Phil. Mag.," April, 1901, p. 503.

The different results with the two metals suggest some chemical interaction between metal and lime. We hope to examine this point later. It was hoped to put such change in evidence by measuring the resistance of the strip day by day. A Crompton potentiometer was used for this purpose, but no change of the order 1 in 1,000 could be found.

Experiments with P.D.s less than the Saturation Voltage.

The experiments at the higher voltages will evidently be influenced by the nature and pressure of the gas, since a large part of the current is carried by ions formed by collisions. With voltages less than that required for saturation the gas carries none of the current.

In one case, typical of the rest when nickel is used, the saturation voltage was 20; the currents were, therefore, measured with 17 and 36 volts. At the lower voltage the current gradually increased over some days to double its initial value, while the usual rise to a maximum and subsequent fall was found with 36 volts. The current due to 17 volts showed no alteration when the apparatus was allowed to stand over night; the current from 36 volts, as already mentioned, showed a large increase.

Using a platinum strip and the lower voltage, the current did not vary by more than 10 per cent. over a month; afterwards a slow increase was observed. No increase or decrease in activity occurred during the night.

Experiments in Flames.

A lime electrode in a flame has been suggested in France as a detector for waves; a few observations on its fatigue may be of interest.

Two platinum electrodes, one of which was coated with lime, were placed in the flame of a Meker burner, and, by means of a suitable battery, the negative ions were driven on to the blank electrode, which was connected to a galvanometer. The current so obtained fell in 10 minutes to one-tenth of its initial value, and then remained more or less steady for the day. The lime had completely recovered its activity next day and went through the same sequence of changes. This large initial rush of current each day is exactly similar to that already found in vacuum tubes at the higher pressures. It was thought it might be due to the oxide absorbing carbon dioxide to form the carbonate ; the electrode was therefore kept on successive nights in air which had been freed from carbon dioxide, in hydrogen and in carbon dioxide, but no difference in behaviour was found.

SUMMARY OF RESULTS.

1. When lime is heated on platinum foil, so far from showing fatigue, it actually increases in activity. With P.D.s greater than the saturation voltage this increase may be nine-fold. At lower voltages a slow but steady increase up to 100 per cent. has been found. The steady activity falls when the lime is cold, the initial activity may, however, greatly increase.

2. With nickel foil, if the tube carries a heavy discharge, the current increases to a maximum and then decreases. A greatly increased activity is frequently shown after the lime has been cold for some hours. At the lower voltages the current shows the same general variations as when platinum is used.

3. Great irregularity is frequently shown on starting the current in a tube that has not been used for some hours. The lime may be at 1,000°C. and yet produce no appreciable current. At this stage the discharge is very sensitive to mechanical vibration. A discharge from a coil greatly increases the activity.

We desire to acknowledge the assistance rendered by Mr. F. G. Bratt during the course of the investigation.

ABSTRACT.

Wehnelt has shown that incandescent lime emits a large number of negative ions ; if, therefore, hot lime is used as the kathode, a discharge may be obtained in a vacuum tube with P.D.s as low as 30 volts. The alteration with time of these kathodes, under continued use, has been investigated and the following results obtained : (1) When lime is heated on platinum foil, so far from showing fatigue, it actually increases in activity. With P.D.s greater than the satura-

tion voltage this increase may be ninefold. At lower voltages a slow but steady increase up to 100 per cent. has been found. The steady activity falls when the lime is cold, the initial activity may greatly increase. (2) When the lime is heated on nickel foil, if the tube carries a heavy discharge, the current increases to a maximum and then decreases. A greatly increased activity is frequently shown after the lime has been cold for some hours. At the lower voltages the same general variations are shown as with platinum. (3) Great irregularity is frequently shown when the current is first started; at this stage other causes than temperature, such as mechanical vibrations, greatly influence the emission of ions.

DISCUSSION.

Prof. C. H. **Lees** asked whether the Author had examined the film under the microscope. He wanted to know whether there was any likelihood of the film getting detached from the platinum and also whether the increase of activity was simply dependent upon the quantity of electricity passed.

Dr. **Willows**, in reply, stated that he had examined the films with a microscope. The films were very adherent to the platinum. It was impossible to render the platinum free again from the activity with which the lime endowed it. There was no connection between the activity and the quantity of electricity passed. The increase of activity was probably due to the CaO diffusing into the platinum.

XXVII. *On the Formation of Dust Striations by an Electric Spark.*

By S. Marsh, *B.Sc., Ph.D., late Fellow of University of Wales, Lecturer in Physics at Battersea Polytechnic; and* W. H. Nottage, *B.Sc., Demonstrator in Physics at Battersea Polytechnic.*

Communicated by W. Thomson. Received March 14, 1911.

§ 1. In the "Phil. Mag." for November, 1909, Mr. T. J. Richmond describes some experiments on the formation of striations in a tube containing a thin layer of fine powder by means of an electric spark which passed between terminals placed at the end of the tube. He showed that the striæ distances varied with the diameter of the tube, and were further dependent upon the type of electric discharge and upon the nature of the powder. The striæ distances for different oscillatory circuits were measured, but the values obtained bore no obvious relation to the frequencies of the electrical oscillations.

In the present Paper the results of an investigation concerning the formation of the striations produced by a spark are given.

It seemed highly probable that the striations produced by the discharge were of the same nature as the striæ seen in Kundt's tube, and experiments were made with a view to investigating this point.

It has been shown by König * that if two spheres be present in a moving fluid at a moderate distance apart they repel one another in the line of motion of the fluid, and attract in a direction perpendicular thereto, and he showed, further, that this offers an explanation of the striations seen in Kundt's tube. The theory has been recently confirmed, and the law governing the striæ distance investigated by Dr. J. Robinson.†

In working with tubes complications arise owing to the reflections of the waves at the walls of the tubes. It was found ‡ that well-defined striations are formed by a spark discharge between two terminals placed over a horizontal plate on which a fine powder is sprinkled. The striations can be obtained on plates of different materials—*e.g.*, glass, ebonite, sheet tin, cardboard, &c. Some preliminary experiments were made with horizontal sparks at various distances above the plates and others where the spark was vertical and at different distances above the plate. Finally, a vertical spark was employed, which passed through a hole bored through the centre of the (glass) plate, the lower terminal being just below the plate.

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* "Wied. Annalen," 42, 353, 549, 1891.

† "Phil. Mag.," July, 1909; April, 1910.

‡ We have recently discovered some records of experiments on striæ formation by an electric spark in various numbers of the "Berichte der Wienerschen Akademie," 1875-8, by Mach, Rosicky and others. They used horizontal sparks and coated the plate with lamp black. These observers attributed the formation of the (few) striations seen to acoustical causes.

§ 2. Investigations were made as to the consequences of the assumption that the periodicity of the sound waves producing the striations was determined by the frequency of the electrical circuit. It was found that the striæ distance was of the same order of magnitude as (and often greater than) the wave-length on this supposition, which fact gives rise to difficulties in the mathematical treatment. We shall refer to these later on. Further, the work of Richmond and a series of experiments performed by ourselves showed no relation between frequency (electrical) and striæ distances.

Now the discharge of electricity across the spark-gap in an oscillatory circuit consists of a number of groups of oscillations separated by relatively large intervals. Moreover, the pulses of the individual groups are heavily damped, and the value of the current, and consequently the heating effect, falls off very rapidly. In order to produce the striations with any degree of clearness and regularity it is necessary to employ a powerful spark, and it seems probable that only the first few pulses in each group are active. In other words, it is supposed that each group of oscillations produces an intense condensational pulse. The induction coil itself gives rise to a quasi-periodicity, in that a certain number of these groups is produced every second. But the fact that the striations is formed by a single group given by a Wimshurst machine and Leyden jar shows that this quasi-periodicity is not material to their formation.

Supposing, then, the heating in the group of oscillations to send out an intense condensational pulse, this would be followed by a rarefaction, and that in turn by a condensation, and so on, a short train of progressive waves being produced, the wave-length of which would depend upon the initial condensation. Concerning this point there are some results to hand. In the well-known experiments of Töpler on the wave sent out by an electric spark, using the so-called "Schlieren Methode," the air pulse appears to be very narrow; but

Rosicky and Mach,[*] using an interferometer method, have shown that the pulse is by no means so " thin " as was supposed. Rosicky concluded from his observations that the condensational pulse often had a thickness of 1 cm. or more. Further, the experiments of Altberg[†] on sound waves of short wave-length do not, in our opinion, speak against this view. The spark he used was very short (0·5 to 1 mm.). Again, his conclusions only hold for $\lambda > 2$ mm., which is a value considerably in excess of that for a Leyden jar discharged through a short wire. Further, in determining the length of the sound waves he used a diffraction grating. The grating will pick out a period in the initial disturbance falling on it, and seeing that each group of discharges has a periodic constitution, the grating transmits the disturbance with this periodicity emphasised. For circuits of very small frequencies the sparks in each group of discharges follow at such relatively long intervals that they can no longer be regarded as culminating together to send out an intense thick pulse. Consequently one would expect to hear a note whose frequency was determined by that of the electrical circuit.

§ 3. We suppose, therefore, that a short train of progressive waves is sent out by the discharge. The length of the spark-gap varied between 1·5 cm. and 2 cm. No advantage is gained by making it longer, for the spark then takes a very irregular path, and, further, is much too " thin " to produce good striæ. Further, we assume that the waves sent are spherical in type (*see* later), and proceed to develop an expression for the interval between consecutive striæ λ and their distances from the spark on that assumption.

§ 4. König has shown (*l.c.*) that the force of repulsion between two spheres of radii a_1 and a_2, which are at rest in a current of velocity w, is equal to

$$-3\pi\rho a_1^3 a_2^3 \cos \quad (3-5\cos^2\theta)\, w^2/r^4,$$

θ being the angle between the line of centres of the spheres and r their distance apart. ρ is the density of the fluid. Where the current is alternating we have to take the mean values of w^2. If w_1 and w_2 are the respective velocities of the current at the spheres, then the mean value of $w_1 w_2$ must be taken.

It should be noted that where the velocities differ in phase by more than a quarter of a period there is a force of *attraction* between the spheres. Where the phase difference is less than a quarter period the spheres repel.

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[*] " Berichte der Wien. Akademie," 1878, 78.

[†] Altberg, " Annalen der Physik," 23, 1907.

Adopting the notation of § 263 in Lamb's "Hydrodynamics" (2nd edition), the differential equation for the propagation of spherical waves is

$$\frac{\partial^2 r\phi}{\partial t^2}=c^2 . \frac{^2(r\phi)}{\partial r^2},$$

ϕ being the velocity potential at a distance r from the centre.

A solution of the equation is

$$\phi=\mathrm{A}\frac{\cos(\sigma t-\kappa r)}{r},$$

where the velocity of propagation $c=\frac{\sigma}{\kappa}$, the wave-length $\lambda=\frac{2\pi}{\kappa}$, and period $\mathrm{T}=\frac{2\pi}{\sigma}$.

The particle velocity $=-\frac{\partial\phi}{\partial r}$, and we have

$$-\frac{\partial\phi}{\partial r}=-\frac{\mathrm{A}\kappa\sin(\sigma t-\kappa r)}{r}+\frac{\mathrm{A}}{r^2}\cos(\sigma t-\kappa r).$$

For values of $\kappa>1$, then, as r increases the second term becomes less and less important, and the velocity falls off as $1/r$.

Let w_1 be the velocity of a particle at a distance, r_1, at time t, and w_2 be the velocity of a particle at a distance, r_2, at time t.

Then
$$w_1=\mathrm{B}\ \frac{\sin(\sigma t-\kappa r_1)}{r_1}$$

and
$$w_2=\mathrm{B}\ .\ \frac{\sin(\sigma t-\kappa r_2)}{r_2},$$

where $\mathrm{B}=-\mathrm{A}\kappa$. The mean value of w_1w_2 over a period T

$$=\frac{\mathrm{B}^2}{\mathrm{T}}\int_0^{\mathrm{T}}\frac{\sin(\sigma t-\kappa r_1)\sin(\sigma t-\kappa r_2)}{r_1r_2}\,dt$$

$$=\frac{\mathrm{B}^2}{2\mathrm{T}}\int_0^{\mathrm{T}}\left[\frac{\cos\kappa(r_2-r_1)-\cos\{2\sigma t-\kappa(r_1+r_2)\}}{r_1r_2}\right]dt$$

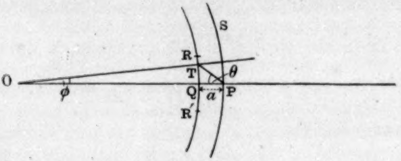
$$=\frac{\mathrm{B}^2}{2}\cdot\frac{\cos\kappa(r_2-r_1)}{r_1r_2},$$

the second integral being zero.

§ 5. We proceed along similar lines to those employed by Dr. Robinson in his discussion of the striations formed in a Kundt's tube.

Assume that a particle in one striation is kept in equilibrium under the forces of repulsion due to the particles of the neighbouring striæ. As the forces vary inversely as the fourth power of the distances between the particles, we only take the nearest striation on either side into account.

Let RQ and PS be portions of two consecutive striæ.



The force of repulsion between a particle at T and one at P is proportional to $\frac{\cos\theta(3-5\cos^2\theta)}{\mathrm{TP}^4}$, and is directed along OP (approximately).

Let n=number of particles per centimetre in the striation RQ. Then the force of repulsion (F) on the particle at P due to the striation RQ

$$=\mathrm{C}\int\frac{\cos\theta(3-5\cos^2\theta)}{\mathrm{TP}^4}\,.\,r\,d\phi\,.\,n,$$

where r=OQ, and C=const.

As the force falls off inversely as the fourth power it is unnecessary to take the whole of the striation QR into account, and we get a sufficient approximation by taking a portion, RR′, such that QR=QR′=a, when PQ=a.

Hence ϕ is a small angle.

The limits of ϕ are $\pm a/r$.

Expressing θ in terms of ϕ, and evaluating the integrals, it is easy to show that $\mathrm{F}\propto(\mathrm{C}\,.\,n/a^3)\times$ constant.

Substituting the value of C, viz., $-3\pi\rho\, a_1{}^3\,.\,a_2{}^3 w_1 w_2$, we see $\mathrm{F}\propto w_1 w_2/a^3$.

[Note.—The approximations used in calculating the force due to the striation QR amount to the same as regarding the portion near P as being straight; hence, the final value is of the same form as that given by Robinson for straight striæ.]

If we have a number of striations of radii $r_1, r_2 \ldots r^n$, then the force on a particle of the second due to the striation at a distance, r_1,

$$\propto \frac{w_1 w_2}{(r_2-r_1)^3} \propto \frac{\cos\kappa(r_2-r_1)}{r_1 r_2 (r_2-r_1)^3},$$

and the force due to the third striation on the same particle of second

$$\propto \frac{w_2 w_3}{(r_3-r_2)^3} \quad \text{or} \quad \frac{\cos\kappa(r_3-r_2)}{r_2 r_3 (r_3-r_2)^3}.$$

For equilibrium we have (assuming n to be the same for all striæ, and that the particles have same radius)

$$\frac{\cos\kappa(r_2-r_1)}{r_1 r_2 (r_2-r_1)^3} = \frac{\cos\kappa(r_3-r_2)}{r_2 r_3 (r_3-r_2)^3}.$$

$$\therefore \quad \frac{r_3-r_2}{r_2-r_1} = \left(\frac{r_1 r_2}{r_2 r_3}\right)^{\frac{1}{3}} \times \left\{\frac{\cos\kappa(r_3-r_2)}{\cos\kappa(r_2-r_1)}\right\}^{\frac{1}{3}}.$$

Also

$$\frac{r_4-r_3}{r_3-r_2} = \left(\frac{r_2 r_3}{r_3 r_4}\right)^{\frac{1}{3}} \left\{\frac{\cos\kappa(r_4-r_3)}{\cos\kappa(r_3-r_2)}\right\}^{\frac{1}{3}}.$$

Finally

$$\frac{r_{n+1}-r_n}{r_n-r_{n-1}} = \left(\frac{r_{n-1} \cdot r_n}{r_n \cdot r_{n+1}}\right)^{\frac{1}{3}} \frac{\cos\kappa(r_{n+1}-r_n)}{\cos\kappa(r_n-r_{n-1})}.$$

$$\therefore \quad \frac{r_{n+1}-r_n}{r_2-r_1} = \left(\frac{r_1 r_2}{r_n \cdot r_{n+1}}\right)^{\frac{1}{3}} \frac{\cos\kappa(r_{n+1}-r_n)}{\cos\kappa(r_2-r_1)}$$

$$= \left(\frac{r_1}{r_{n+1}}\right)^{\frac{2}{3}} \frac{\cos\kappa(r_{n+1}-r_n)}{\cos\kappa(r_2-r_1)} \text{ approx.}$$

With regard to the cosine term, the following considerations show that it is sensibly unity :—

$$\kappa = \frac{2\pi}{\lambda}. \quad \text{For } \lambda = 1 \text{ cm. approx. } \kappa = 2\pi.$$

Taking average values for the striæ distances close in and far out we may put

$$r_2 - r_1 = \frac{10}{200} \text{ cm. and } r_{n+1} - r_n = \frac{6}{200} \text{cm.},$$

whence we have

$$\frac{\cos 2\pi \cdot \frac{6}{200}}{\cos 2\pi \cdot \frac{10}{200}} = \frac{\cos 11^\circ}{\cos 18^\circ} = \frac{0{\cdot}982}{0{\cdot}95}.$$

Hence, approximately,

$$\frac{r_{n+1}-r_n}{r_2-r_1} = \left(\frac{r_1}{r_{n+1}}\right)^{\frac{2}{3}}.$$

EXPERIMENTAL DETAILS.

§ 6. In most of the experiments an induction coil was used and various condensers inserted in the discharge circuit. The striations were measured by a travelling microscope fitted with a micrometer eyepiece scale (20 divisions per millimetre), and, where not otherwise stated, the striæ distances are given in terms of the micrometer divisions. It is remarkable how well on the whole the striae lend themselves to measurement, although irregularities are to be noted in places where the striæ are much forked, as a fork modifies the neighbouring striæ considerably. The number visible in the eyepiece was noted and divided into the total distance on the scale. The value obtained was taken as the average distance apart for the distance from the centre given by the travelling microscope. The average distance between the striæ was obtained every few millimetres, and as far out from the centre as possible.

In comparing the results obtained with those given by the theory some care is necessary. If, for example, the intervals between consecutive striæ for different values of r are compared with the interval for a particular value of r, *i.e.*, if in the notation here used the striæ intervals are compared with the striæ interval $r_2—r_1$ at a distance r_1 from the spark, then if there is any irregularity in $r_2—r_1$ due to the presence of forks in the neighbourhood, this error will be communicated to all. The results are, therefore, worked out in two ways. First, the value of the striæ interval at a place where the striations are well formed is taken as origin, and all others referred to it. Secondly, the results are taken in pairs 1, 2 ; 2, 3 ; . . .

In Table I. a set of values obtained is given :—

TABLE I.

r cms.	Mean striæ distance.	Observed values.	Calculated values.	Observed values.	Calculated values.
		$\frac{r_{n+1}-r_n}{r_2-r_1}$	$\sqrt[3]{\left(\frac{r_1}{r_{n+1}}\right)^2}$	$\frac{r_{n+1}-r_n}{r_2-r_1}$	$\sqrt[3]{\left(\frac{r_1}{r_{n+1}}\right)^2}$
7·1	6·0	...	...		
6·8	6·25	0·96	0·97	0·96	0·97
6·32	6·88	0·87	0·93	0·91	0·95
5·88	6·63	0·91	0·88	0·94	0·91
5·4	7·06	0·85	0·83	0·94	0·95
5·0	7·38	0·81	0·79	0·96	0·95
4·7	7·8	0·77	0·76	0·95	0·96
4·03	8·3	0·72	0·69	0·94	0·90
3·66	10·0	0·60	0·64	0·83	0·94
3·27	10·8	0·56	0·60	0·93	0·93
2·74	11·1	0·54	0·53	0·97	0·89

In columns 3 and 4 of the table $r=7{\cdot}1$ is chosen as the distance with which all the others are compared. In columns 5 and 6 the results are compared pair by pair.

We give below another set of values for a different experiment:—

TABLE II.

r cms.	Mean striæ distance.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
		$\frac{r_{n+1}-r_n}{r_2-r_1}$	$\sqrt[3]{\left(\frac{r_1}{r_{n+1}}\right)^2}$	$\frac{r_{n+1}-r_n}{r_2-r_1}$	$\sqrt[3]{\left(\frac{r_1}{r_{n+1}}\right)^2}$	$\frac{r_{n+1}-r_n}{r_2-r_1}$	$\sqrt[3]{\left(\frac{r_1}{r_{n+1}}\right)^2}$
3·41	12·5	0·39	0·46	1·67	1·39	0·77	0·82
4·63	9·6	0·51	0·57	1·28	1·13	0·78	0·88
5·6	7·5	0·65	0·64	...	...	0·95	0·93
6·29	7·1	0·69	0·69	0·95	0·93	0·86	0·94
6·95	6·13	0·80	0·74	0·82	0·87	0·92	0·88
8·43	5·65	0·86	0·84	0·75	0·76	1·00	0·97
8·85	5·65	0·86	0·87	0·75	0·74	0·92	0·96
9·41	5·2	0·94	0·91	0·69	0·71	0·96	0·97
9·91	5·0	0·98	0·94	0·67	0·68	0·98	0·94
10·9	4·88	...	...	0·65	0·64		

In columns 3 and 4 $r=10{\cdot}9$ is chosen as origin. In columns 5 and 6 $r=5{\cdot}6$ is chosen as origin. In columns 7 and 8 the readings are taken pair by pair.

The preceding tables show that there is, on the whole, a good agreement between the experimental values and those calculated from the theory. The differences are of the same order as the errors of observation. A better agreement could not well be expected, for the wave given by the spark is only approximately spherical in type. Moreover, there is some uncertainty in determining the striæ interval where the striæ are forked or badly formed; again, the action of the nearest striæ only has been taken into account in the theory, and viscosity and other frictional forces have been left out of consideration.

With particles of comparatively large dimensions, such as sand or emery powder, the striæ are in the form of chains of particles placed end to end. A photograph of sand striations, magnified 20 diameters (*see* Fig. 1, Plate), shows very clearly how the particles arrange themselves in line due to the attraction between them in a direction perpendicular to that of propagation of the wave.

If the spark be horizontal and just above the surface of the plate, the striations extend only a short distance in the direct tion of the spark-gap, but much further in a perpendicular direction (Fig. 2). Circular arcs with centre in the mid point of the spark-gap have been struck, and it will be noticed that the striations are practically concentric with them.

EXPERIMENTS ON STRIATION FORMATION IN CHANNELS.

§ 7. The investigation in § 5 shows that the force of repulsion between two striations varies as $w_1 w_2/a^3$. Now, for a plane wave the amplitude of the particle velocity is independent of the distance from the source, and we should expect the striae distance to be constant also. If the wave be confined by parallel walls, continued reflections at the walls will render the wave approximately plane, and hence the striæ distance should be approximately constant. To test this channels were formed in the plate by laying strips of glass or wood parallel to each other at various distances apart. The striations formed, especially those in the narrow channels, were very well defined and extended much further from the source than those formed by the same spark when no channels were present. A set of values are given below. The spark was the same length throughout, and in the first three cases the tops of the channels were left open.

Channel width.	Striæ distance.
2·5 cm.	6·9
1·5 cm.	10·3
0·9 cm.	12·3
1·65 cm. covered channel	11·1

In the case of the narrowest channel the spark was passed for a considerable time. The above results are the mean values of the striæ distances over the whole length of the channel. The striæ distance was not constant, but fell off slowly as the distance from the spark increased. It was found that, other conditions being the same, the striæ were further apart in covered channels than in open ones of the same width.

The experiments made with channels of different widths and with rectangular tubes give the same results as those made by Richmond for tubes of circular section. For the same spark the striæ distance is a maximum for a certain width. With narrow channels the whole of the section is covered by uniform striations, perpendicular to the walls; but for wider channels there is a well-defined central pattern and also side patterns, but in the portions of the channel on either side of the central pattern the striæ are not well formed (Fig. 3). In working with ordinary tubes it is evident that the central pattern only is obtained. The fact that the striæ intervals vary with the channel and tube is perhaps to be ascribed to the reflections which take place at the walls. These will reinforce one another at the centre if the spark be opposite the middle of the channel, and the extent of reinforcement will vary with the size of the tube.

Experiments have been made with ordinary tubes with the spark-gap in the first place opposite the end of the tube (as in Richmond's experiments). Secondly, T pieces were blown on the tube and the terminals introduced through these into the tube and sealed with wax. It was found that the character of the striations depended upon whether the ends were open or closed. In one tube, for example, one end of which was sealed off a short distance from the T piece, the motion was very energetic, and during the passage of the spark the powder gathered itself up in little clouds about 1 cm. apart. On closing the open end with the hand or a cork the striæ were much closer together, and were similar to those obtained when the spark-gap was at the end of an open tube. It seems probable that stationary wave motion is set up with certain end conditions, and the powder gathers itself up into clouds at the nodes. The wave-length of this disturbance, calculated from the nodal distances would agree very well with the value ascribed to the wave-length earlier in the Paper. It was found that the striæ distance falls off slowly as the distance from the spark increases, which agrees with the experiments with channels. This decrease is without doubt due to the diminution in the amplitude owing to viscosity, &c.

EXPERIMENTS WITH DIFFERENT OBSTACLES IN THE PATH OF THE WAVE.

§ 8. Some interesting effects have been obtained by placing various obstacles on the plate near the spark-gap. With a plane obstacle formed of glass or wood strips laid across the plate the striations near the central portion of the obstacle are parallel to the " wall." At the sides short striæ perpendicular to the wall are obtained (Fig. 4). The striæ distance falls off in going outwards from the spark, but becomes much greater close to the wall. This is doubtless caused by the increase in amplitude of the disturbance close to the reflector owing to the superposition of incident and reflected waves. Further from the centre of the obstacle the incident and reflected waves give rise to a wave motion along the face of the obstacle and produce perpendicular striæ. The formation of striations in this way provides an excellent means of illustrating the reflection and interference of waves diverging from a small source. Where two waves cross one another we have a "tracery" pattern, and where they reinforce well-defined striations. If the spark-gap is arranged at one focus of an ellipse, the converging of the striations round the second focus and the heaping up of the powder there is very apparent. Fig. 4 illustrates the bending of the waves round small obstacles. They illustrate the diffraction of aerial waves of short wavelength. The pattern between the small cylinders closely resembles the lines of force between two electrically charged cylinders or magnet poles, and affords an interesting illustration in support of the analogy between the hydrodynamical and magnetic forces.

The striations obtained in a channel formed by walls making an angle with each other are worthy of some remarks. When the spark is opposite the middle of the wide end, then a well-defined central pattern is formed slightly concave to the spark. At the sides perpendicular striæ are formed, while in between striations parallel to the sides are formed (Fig. 3). The central pattern is formed by reinforcement along the centre line of the wave reflected from the sides.

Regular polygons were formed by glass strips, the spark-gap being at the centre of the figure. Passing from the spark to each angular point and to the centre of each side a narrow strip of striations were obtained. Around the sides of the polygon short striations perpendicular to the sides are formed (*see* Fig. 5).

§ 9. The striations from which measurements were taken were formed in lycopodium. Cork dust gives clear and steep striations very suitable for photography. Amorphous silica gives ill-defined striations, apparently due to a cloud of the fine powder settling on them. Sand and other coarse-grained powders form chains of single particles which do not extend outwards for such great distances as do those produced with smaller particles. By using sand in a narrow channel closed at the end and focussing a low power microscope upon the grains the formation of the striations can be watched. It is interesting to see a grain move up to a striation and take its place in the chain, and the repulsive force between particles in neighbouring chains was clearly indicated.

FIG. 1.—PHOTOMICROGRAPH OF SAND GRAINS.

To face p. 274.

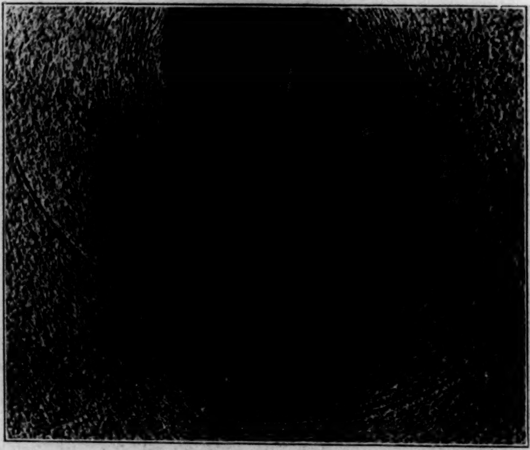


Fig. 2.—Striations with Circular Arcs.

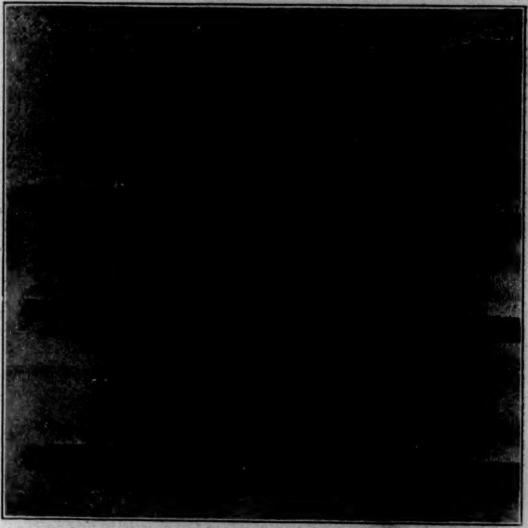


FIG. 3.—STRIATIONS IN LYCOPODIUM—CHANNELS.

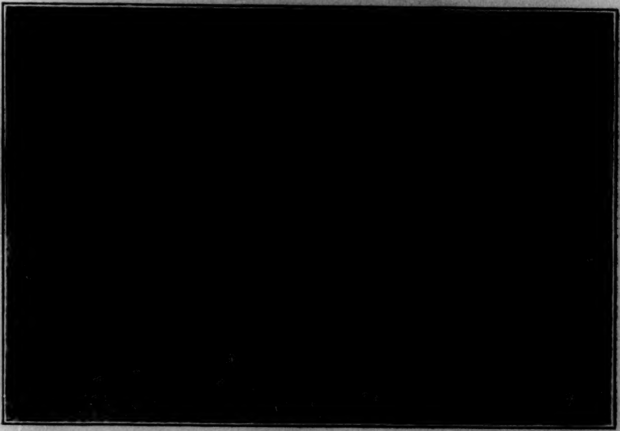


FIG. 4.—STRIATIONS IN CORK—SMALL OBSTACLES.

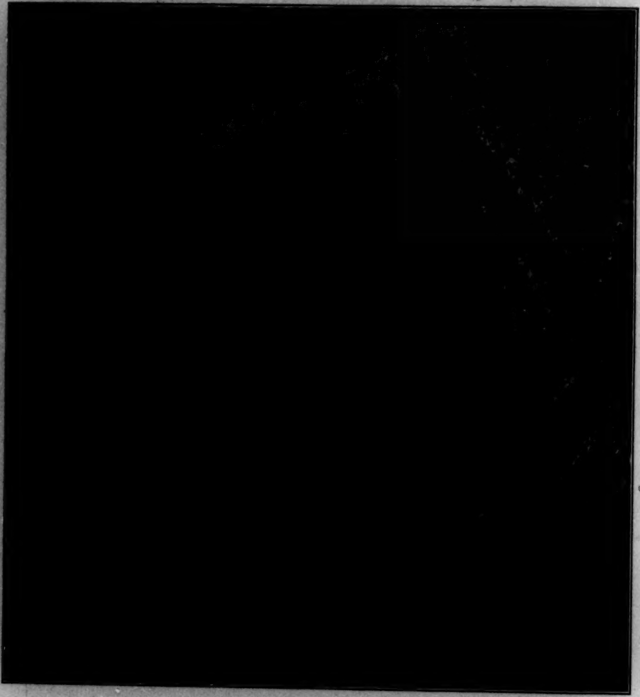


Fig. 5.—Striations in Cork—Closed Polygon (Pentagon).

The form of the cross-section of the striations was obtained in the following manner :—

A plate was cut into two pieces along a line passing through the central hole. These pieces were carefully fitted together, being laid on another clean plate, so that the edges were flush. Striations were formed in the ordinary way, and the two parts carefully separated and viewed by a microscope of moderate power. The sides of the striations were fairly steep (with cork dust they are nearly vertical), and their height about equal to the distance between them.

[Note.—During the writing up of this Paper a short note by Dr. Robinson appeared in the "Phil. Mag." for February, in which the suggestion was made that the striæ produced by a spark are of the same nature as those obtained in a Kundt's tube.]

We desire to express our best thanks to Mr. Thomson, M.A., B.Sc., for his valued criticism and interest in the research.

[Note, April 6th.—Our attention has been drawn to a Paper by Dr. Cook in the "Proceedings" of the Society (1888), illustrating the formation of dust striations on a plate by a horizontal spark. In a footnote Cook refers to an experiment due to Guthrie in which an elliptical reflector was used and a spark arranged at one focus, whereby striations similar to those shown in Fig. 5 were obtained.]

ABSTRACT.

The formation of dust striations by electric spark has been investigated by many observers. The Paper attempts to explain their formation as being due to hydrodynamic forces existing between the dust particles while the wave motion is passing over them. The application of this theory to the striations in a Kundts tube has been made by Kœnig and Robinson. The wave motion is assumed to be of the spherical progressive type and expressions are obtained for the intervals between consecutive striæ and the distances of the striæ from origin. Measurements were made of striæ formed on a glass plate with vertical central spark. The agreement between theory and experiment is within the experimental error. Experiments with channels of various shapes were made. Illustrations of the various striæ patterns obtained with small obstacles and reflecting surfaces are given, and the use of these as a convenient means of indicating reflection interference and diffraction of sound waves is pointed out.

DISCUSSION.

Mr. A. Campbell thought the authors' results justified their theory. In the experiments recently described by himself and Mr. Dye there was fair correspondence between the actual oscillation frequency and the closeness of the dust streaks. The frequencies used (50,000 up to 700,000 ~ per second) were very much smaller than those used by the authors (100 million per second), while the damping was not considerable, at least 25 to 30 oscillations taking place in each spark train. Thus it might be expected that very different effects would occur in the two cases.

Dr. **Russell** said that the formation of striations was one of the best methods of telling whether a spark was oscillatory and of high frequency or not. All that was necessary was to sprinkle a little lycopodium or other light powder on a glass plate or on the surface of water and hold it near the spark. If the spark were oscillatory circular striations would be at once formed. He suggested that the method might be used as a rough test for oscillations in the same way that chemists use litmus as a test for acids. He had carried out experiments on the striations formed in long glass tubes of various diameters. The distances between the striations varied with the frequency of the spark. He was struck by the fact that the distance between them was, except near the ends, practically constant along the whole length of the tube. Closing one end of the tube or varying the length had very little effect on this distance. The striations were always in motion, and the complete theory must be very complex. He thought that the best way to produce oscillatory sparks for these experiments was to use an induction coil with two Leyden jars connected across its secondary terminals in Lodge's manner. He congratulated the authors on the interesting theoretical results they had obtained.

Dr. **Marsh**, in reply, stated that the experimental results of Richmond and the authors showed that there was no relation between the striæ intervals and oscillation frequencies. He referred to a note in the "Philosophical Magazine" for February by Dr. Robinson, who performed a series of experiments in which the frequency was kept constant, but the intensity of the spark was varied. The results showed that the striæ intervals *increased* with the intensity of the spark, so proving that the striations are not formed at the nodes of a secondary wave motion set up in the tube. In the authors' experiments the striæ were formed by progressive waves, and this shows clearly that the explanation must be looked for in another direction. The frequency was varied within wide limits, but no marked change was observed in the striæ formed on the plate. In long tubes they found that the striæ intervals became slightly smaller with increasing distance from the spark end.

XXVIII. *The Method of Constant Rate of Change of Flux as a Standard for Determining Magnetisation Curves of Iron.*

By J. T. Morris *and* T. H. Langford, *B.Sc.*

Communicated by Prof. C. H. Lees, F.R.S. Received March 22, 1911.
Read May 12, 1911.

Introduction.

Doubts must have arisen in the minds of many investigators as to how nearly the magnetisation curves of a sample of iron obtained by differing methods would agree with one another.

Discrepancies which have been found from time to time may be attributed to one or more of the following causes :—

(i.) Inaccuracies in experimenting.

(ii.) Lack of exact repetition of previous conditions. In other words, the physical state may differ in different experiments with regard either to hardness, temperature or previous magnetic treatment.

(iii.) Differences inherent in the methods employed.

The present research was instituted with the object of studying the last of these three causes, and obtaining definite information as to the magnitude of these differences (*a*) between the magnetisation curves for a given sample of iron when determined by the older methods, in which the flux is changed suddenly, and (*b*) by a method in which it is changed slowly and at a uniform rate.

Methods in General Use.—The relation between the magnetising force **H** and the flux-density **B** produced by it in a specimen of magnetic material is usually determined by one of the following methods :—

(*a*) *Ballistic Methods.*—The specimen, preferably in the form of a closed ring, is wound with primary and secondary coils, and the change of flux, caused by a sudden change of current in the primary coil, is measured by the extent of the first swing given by the moving system of a ballistic galvanometer in series with the secondary winding.

(*b*) *Alternate-current Methods.*—The specimen is prepared as in (*a*) with primary and secondary coils, and alternating currents of commercial frequency are passed through the primary winding, the maximum currents and the corresponding maximum values of the flux-density being determined.

(*c*) *The "Fluxmeter" Method.*—The Grassot fluxmeter* is an instrument somewhat similar to a ballistic galvanometer, but the moving coil is suspended by a single cocoon fibre exerting no appreciable controlling torque. The movable coil is connected to the secondary winding of a specimen ring, as in the ballistic method, and it is deflected through an angle which is proportional to the time-integral of the voltage induced in the secondary winding. The deflection is thus proportional to the total change of flux through the ring, and is independent of the time occupied by the change.

(*d*) *Magnetometric Methods.*—A long thin bar of the material to be tested is wound with a magnetising coil, and the strength of pole produced by a known number of ampere-turns is determined by means of a magnetometer.

(*e*) *Attraction Permeameter Methods.*—The sample is arranged to close the magnetic circuit of an iron yoke of such dimensions that its reluctance may be neglected; then the mechanical force of attraction between the specimen and the yoke is measured by observing either the force necessary to detach the specimen or the increase of the force required to cause the specimen to slide on the yoke.†

COMPARISON OF METHODS.

Permeameter measurements have the advantage of rapidity, but the accuracy attainable is not very high, owing to the indefinite degree of contact between the specimen and the yoke. This method is generally employed only for workshop tests. The magnetometric method is liable to inaccuracies due to the lack of uniformity in the magnetisation of the sample, including the indeterminate position of the poles and their demagnetising action. Alternate-current methods have the disadvantage that they are indirect, the required values of the current and flux-density having to be deduced from the observations by a complicated method if accuracy is to be ensured. When the greatest accuracy is required, ballistic methods on ring samples are, therefore, those to which resort is usually made. The test is commonly made by one of two methods: In one of these the current through the primary winding is increased up to a certain value and then suddenly reversed to obtain a reading ; in the other method the current is increased or diminished by a step at a time, and the consequent ballistic reading taken. This difference in the character of the magnetic change to which the iron is subjected is liable to cause differences in the results obtained, partly owing to magnetic " creep " or viscosity.

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* "Some New Electrical Instruments," THE ELECTRICIAN, Vol. LVI., p. 560, 1906.

† S. P. Thompson, "The Electromagnet"; W. H. F. Murdoch, "The Magnetic Testing of Iron," I.E.E., Vol. XL., p. 137, 1907.

A method proposed by C. F. Scott, and apparently first described by J. S. Peck,* as applied to large transformers, possesses the marked advantage over ballistic methods that the variation of the flux takes place at a known and constant rate, enabling experiments to be repeated under exactly similar conditions. Rates of change varying from 50 to 250 lines per square centimetre per second were actually employed in the experiments. Further, standardisation can be more readily performed, as a mutual induction is not required. This method has been modified by Dr. D. K. Morris so as to render it practicable for tests on small samples of iron.†

The Method of " Constant Rate of Change of Flux."—Consider an iron ring in which a flux, N, is produced by a current, C, flowing in a primary winding. By varying the current C in a continuous manner the flux N in the ring is continuously varied, and at any instant the E.M.F. E induced in a secondary winding is proportional to the rate of change of the flux N—*i.e.*, to dN/dt—and the total change of flux which has taken place in the ring during any interval of time (t_2-t_1) is N_2-N_1 $=\int_{t_1}^{t_2}\frac{dN}{dt}\,.\,dt.$

If now the primary current C be varied at such a rate that the induced E.M.F. E is maintained at a constant value, the flux N is known to be varying at a constant rate, $dN/dt=k$.

In this case the total change of flux through the ring is simply proportional to the time during which it has taken place, for

$$\int_{t_1}^{t_2}\frac{dN}{dt}\,.\,dt=k\int_{t_1}^{t^2}dt=k(t_2-t_1).$$

If n_2 be the number of turns on the secondary winding, and

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* J. S. Peck, "On Testing Large Transformers," "Electrical World and Engineer," Vol. XXXVII., p. 1083, 1901.

† D. K. Morris and G. A. Lister, " Transformers and Transformer Iron," I.E.E., Vol. XXXVII., pp. 282-294.

E be the value of the constant E.M.F. in volts induced therein, we have

$$E = dN/dt \times n_2 \div 10^8 = kn_2/10^8$$

$$\therefore \quad k = \frac{E \times 10^8}{n_2},$$

and total change of flux

$$(N_2 - N_1) = k(t_2 - t_1) = \frac{E \times 10^8}{n_2}(t_2 - t_1).$$

Hence a magnetisation curve can be deduced from readings of current and time when the current is varied at such a rate that the secondary induced E.M.F. is constant and of known value.

Since the rate at which the flux is varied can be made as slow as desired, any "creep" in the magnetisation due to magnetic viscosity will be recorded. This method, therefore, appears to be preferable to others as the standard method for determining the fundamental magnetic properties of iron.

The following experiments were undertaken to investigate the differences, if any, between the results obtained by this and by other methods.

Methods Experimentally Examined.

I. *Method of Constant Rate of Change of Flux, or, more shortly, that of "Uniformly-varying Flux"* (or "Constant Induced Voltage," as it has been termed by previous writers).—After the iron has been demagnetised, the flux-density is continuously raised from zero to the maximum value, as above described.

II. "*Slow Cyclic*" *Hysteresis Loops.*—A similar method to the first, except that the iron is carried through cycles of magnetisation of gradually increasing maximum value, and the maximum values of **B** and **H** are plotted to give the magnetisation curve.

III. "*Step-by-step*" *Magnetisation Curve.*—The iron is first demagnetised and the current is then raised by small increments, the flux-density produced being deduced from the throws observed on a ballistic galvanometer after each increment of current.

IV. "*Step-by-step*" *Hysteresis Loops.*—A similar method to the last, except that the iron is carried through cycles of magnetisation of gradually increasing maximum value, and the maximum values of **B** and **H** are then plotted to give the magnetisation curve.

V. *Method of Reversals.*—The iron is demagnetised, the current raised to a certain value, reversed, and the resulting throw taken on a ballistic galvanometer. This is repeated for currents of gradually increasing value, and the magnetisation curve so obtained plotted.

VI. *Alternating-current Magnetisation Curve.*—The magnetisation curve is deduced from readings of the magnetising current and induced E.M.F. when the winding of the ring is traversed by an alternating current at a frequency of 60 cycles per second.

Before each reading recorded by methods II., IV. and V. the iron was brought into a cyclic condition by carrying it through a sufficient number of cycles of magnetising force of the same maximum value, some 50 cycles being generally sufficient for this purpose.

PRACTICAL DETAILS OF METHOD OF UNIFORMLY VARYING FLUX.

In Fig. 1 is given a diagram of connections. The voltage induced in the secondary winding S is balanced by the fall of potential over a known resistance, r_1, in series with a known resistance, r_2, across a cell, B, of known E.M.F., equality being indicated by a zero reading on a galvanometer, G. The battery B is closed on to the circuit $r_1 r_2$, and simultaneously the primary current is caused to begin to change by means of the rheostat A, and the experimenter has continuously to adjust the rheostat at such a pace as will maintain the galvanometer reading at zero. This adjustment requires some skill, but an average experimenter easily acquires this with a little practice.

In making a hysteresis test the primary current is continuously varied from a maximum positive value to an equal negative value by a specially designed resistance,* in which two sliders, *a*, *b*, connected to the primary winding P, are moved in opposite directions across the edges of the zigzag strip resistance A. A photograph of the rheostat employed is shown in Fig. 2.

The primary current is measured on a potentiometer by the voltage drop across a known resistance, r_3, the exact times at which the current has certain convenient values being recorded by a chronographic arrangement.

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* J. T. Morris, R. M. Ellis and F. Stroude, "The Design of a Continuously Adjustable Rheostat," THE ELECTRICIAN, Vol. LXI., p. 400, 1908.

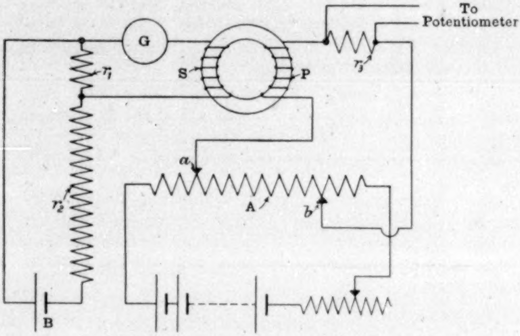

FIG. 1.—CONNECTIONS FOR METHOD OF UNIFORMLY VARYING FLUX.

A Morse inker was employed for this purpose, the receiving coil being in series with a contact made at each swing of a

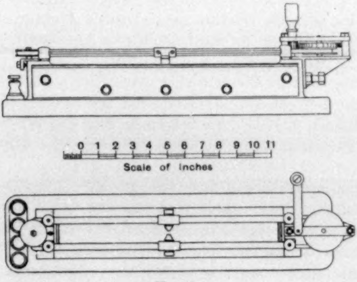

FIG. 2.

pendulum. The arm carrying the inking wheel was arranged to deflect a pencil (which rested on the travelling paper tape) a small distance laterally. A key was also arranged so that, on depression, the same pencil was deflected laterally double the distance. Timing and observation marks were easily distinguishable. This arrangement was found to be quite satisfactory.

The above electrical connections could be simplified if the galvanometer were directly closed on to the terminals of the secondary winding, but this arrangement has some objections—(1) that it is difficult to allow for the uncertainty in the value of the secondary E.M.F. induced during the time that the galvanometer deflection is attaining its normal magnitude; and (2) that a separate operation has to be performed in order to standardise the galvanometer. A further slight objection is that there is an opposing magnetomotive force due to the current flowing in the secondary winding. This, in general, however, is so small as to be negligible.

Magnetisation Curve by Alternating Current.

It is important in connection with transformers and other alternating-current apparatus to determine the maximum value of the alternating flux-wave produced in iron by a magnetising current wave of known maximum value.

In order to carry out this determination the following experiments were made. The iron ring was demagnetised by a continuously decreasing alternating current from a generator, G (Fig. 3). This current was then gradually increased, its R.M.S. value being read on an ammeter, A, and the R.M.S. value of the voltage induced in the secondary winding S measured by the deflection produced on a Duddell thermo-galvanometer, TG, in series with a known variable resistance, R_2. The thermo-galvanometer was calibrated by a secondary cell, B, of known E.M.F. To determine the wave forms of the magnetising current and secondary induced voltage the strips CS and VS of a Duddell oscillograph were connected as shown, the current strip CS being shunted by a variable portion of a resistance, R_1, and the volt strip VS being connected so as to measure the volts across the primary winding P, the ammeter A and the current strip CS in parallel with its shunt. The resistance of this circuit was measured, and the measured volt wave corrected by subtraction of the corresponding $C \times R$ drop, thus giving the true induced E.M.F. wave. The area of this was then measured by a planimeter, and from this the maximum flux-density was calculated, corresponding to the R.M.S. volts measured on the thermo-galvanometer. This determination was made for a number of different values of the secondary induced voltage. As a check, the R.M.S. value of the corrected volt wave was determined, and compared with the value obtained by measurement with the thermo-galvanometer on the secondary; also the R.M.S. value of the volts measured by the volt strip VS was read on the thermo-galvanometer and compared with the value obtained from the oscillogram.

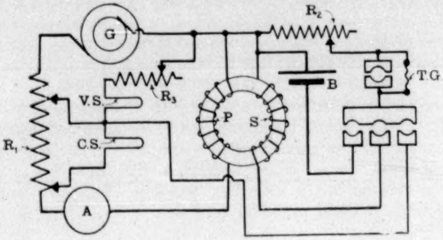

FIG. 3.—CONNECTIONS FOR MAGNETISATION CURVE BY ALTERNATING CURRENT.

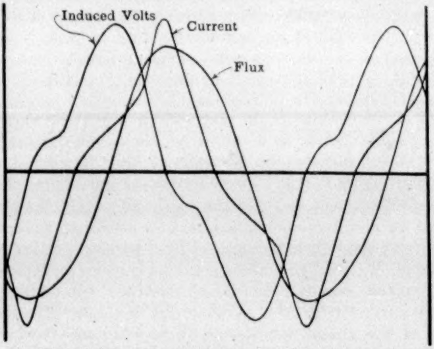

FIG. 4.—FLUX WAVE DEDUCED FROM OSCILLOGRAM.

A specimen oscillogram, showing current and induced voltage waves, together with the flux wave deduced therefrom, is shown in Fig. 4.

The value of the maximum flux-density induced may be determined from the R.M.S. value of the induced voltage by the formula

$$\mathsf{B}=\frac{E}{Z\times n\times A}\times k\times 10^8,$$

where E=R.M.S. value of induced voltage.
Z=Number of secondary turns.
n=frequency in cycles per second.
A=cross-sectional area of iron ring in square centimetres.
B=maximum flux-density in lines per square centimetre.
k=factor dependent on the wave-form, being $1/4{\cdot}44 = 0{\cdot}2252$ for a sine wave of induced voltage.

The values of the factor k were experimentally determined at various points on the magnetisation curve, and are given in Table I.

TABLE I.—*Values of Factor " k " in Formula* $\mathsf{B}=\frac{E\times 10^8}{ZnA}\times k$.

R.M.S. volts generated per turn at 1 period per second.	Factor " *k*."
0·000129	0·227
0·000422	0·228
0·00133	0·211
0·00134	0·217
0·00166	0·192
0·00171	0·193

The maximum value of the magnetising current is obtained from its R.M.S. value as measured on the ammeter by multiplying this figure by the form-factor, the experimentally-determined values of which are collected in Table II.

TABLE II.—*Current Wave Form Factors.*

R.M.S. amperes.	Form Factor.
0·194	1·430
0·405	1·370
1·854	1·904
2·57	1·935
5·05	1·820
8·135	2·070

Data of Specimen used.—The ring was made up of 24 stampings, 6 in. external and 5 in. internal diameter, of Sankey's best transformer iron, manufactured about the year 1900, of

24 S.W.G. The mean magnetic length of the ring was found by measurement to be 43·93 cm. The sectional area of the ring was determined from its weight and specific gravity, the latter being determined by weighing in air and in carbon tetrachloride. This liquid is preferable for this purpose to water on account of its non-oxidising properties, and also because of its higher specific gravity. The stampings were insulated from one another by thin paper rings, and the complete ring was lapped over with insulating tape and wound with two No. 19 D.C.C. wires laid side by side, giving two windings of 147 turns each. These could be used in series or in parallel as a magnetising winding, or one could be used as a magnetising winding and the other as a secondary. A secondary winding of 50 turns and one of 10 turns were also wound on. Exact data of the stampings are as follow :—

Weight of pack of 24 stampings .... = 653·0 grm.
Volume (sp. gr. = 7·805) .... = 83·67 c.c.
Mean length of path .... = 43·93 cm.
Sectional area .... = 1·904 sq. cm.
Effective thickness .... = 1·470 cm.
Actual thickness of laminæ + paper insulation ...... = 1·616 cm.

The paper insulation occupies about 5 per cent. of the total thickness, and the roughness of the iron surface accounts for about 4 per cent.

EXPERIMENTAL RESULTS.

The results obtained experimentally by the above-described methods I. to VI. are given in the form of curves in Figs. 5, 6 and 7, and readings taken from the plotted curves are given in Tables III. and IV.

The magnetisation curves are shown in Fig. 5, the initial parts being shown separately to an enlarged scale of **H**.

In Fig. 6 the results are shown in the form of permeability curves, and in Fig. 7 in the form of reluctivity curves.

In Fig. 8 the differences between the flux-density obtained by method I. and by methods II., III., IV. and V., with the same magnetising force, are plotted to a greatly enlarged scale against **B** as obtained by method I. The divergence of the magnetisation curve obtained by method VI. from the standard curve obtained by method I. is too great to allow of its being shown in Fig. 8, but is apparent in Figs. 5, 6 and 7.

Fig. 9 shows hysteresis loops taken by methods II. and IV., the maximum value of **H** being the same in each case.

The above experiments were carried out in the electrical

TABLE III.

H	Flux Density B by methods :—					
	I.	II.	III.	IV.	V.	VI.
0·3	360	100	120	100	210	140
0·5	610	220	350	200	400	320
0·7	990	500	750	400	750	610
0·9	1,530	1,080	1,300	900	1,230	950
1·2	2,680	2,500	2,500	2,300	2,400	1,620
1·5	3,840	3,700	3,650	3,600	3,620	2,500
2·0	5,450	5,600	5,100	5,250	5,300	4,000
2·5	6,560	6,750	6,300	6,350	6,550	5,400
3·0	7,480	7,700	7,250	7,200	7,400	6,550
3·5	8,250	8,420	8,020	8,020	8,120	7,650
4·0	8,850	9,080	8,700	8,600	8,700	8,500
4·5	9,440	9,600	9,200	9,060	9,300	9,220
5·0	9,880	10,100	9,620	9,500	9,800	9,800
6·0	10,700	10,920	10,410	10,300	10,500	10,650
7·0	11,330	11,550	11,050	11,040	11,250	11,300
8·0	11,920	12,100	11,600	...	11,850	11,820
9·0	12,400	12,600	12,100	...	12,300	12,300
10·0	12,850	12,960	12,520	...	12,750	12,760
12·0	13,580	13,600	13,110	...	13,400	13,460
14·0	14,200	14,100	13,590	...	13,950	14,200
16·0	14,720	14,510	13,950	...	14,380	14,620
18·0	15,110	14,750	14,300	...	14,750	15,100
20·0	15,400	15,100	14,650	...	15,050	15,450
25·0	16,040	15,510	15,440	...	15,600	16,120
30·0	16,500	16,050	16,010	...	15,830	16,420
35·0	16,840	...	...	...	16,180	16,650
50·0	17,320	...	...	...	16,700	17,020
70·0	17,850	...	...	...	17,270	17,120

TABLE IV.

B	Permeability by methods :—					
	I.	II.	III.	IV.	V.	VI.
500	1,210	740	820	660	880	840
1,000	1,430	1.140	1,240	1,070	1,245	1,075
1,500	1,668	1,510	1,580	1,440	1,545	1,300
2,000	1,950	1,810	1,870	1,750	1,820	1,470
2,500	2,175	2,080	2,080	2,015	2,030	1,630
3,000	2,356	2,270	2,270	2,215	2,220	1,755
3,500	2,500	2,420	2,400	2,370	2,380	1,850
4,000	2,660	2,535	2,480	2,490	2,505	1,940
4,500	2,690	2,640	2,520	2,580	2,590	2,000
5,000	2,740	2,730	2,545	2,620	2,650	2,050
5,500	2,740	2,787	2,555	2,620	2,660	2,090
6,000	2,700	2,780	2,550	2,585	2,645	2,130
6,500	2,620	2,740	2,520	2,520	2,620	2,150
7,000	2,565	2,660	2,465	2,440	2,550	2.160
8,000	2,410	2,490	2,295	2,270	2,360	2,156
9,000	2,190	2,255	2,070	2,035	2,120	2,045
10,000	1,930	2,010	1,830	1,810	1,790	1,910
11,000	1,720	1,765	1,590	1,570	1,660	1,720
12,000	1,465	1,510	1,345	...	1,440	1,500
13,000	1,250	1,260	1,100	...	1,210	1,280
14,000	1,060	1,005	860	...	980	1,070
15,000	868	765	660	...	760	880
16,000	650	520	530	...	520	650
17,000	425	...	...	...	280	330

engineering department of the East London College (University of London), and the authors wish to express their best thanks

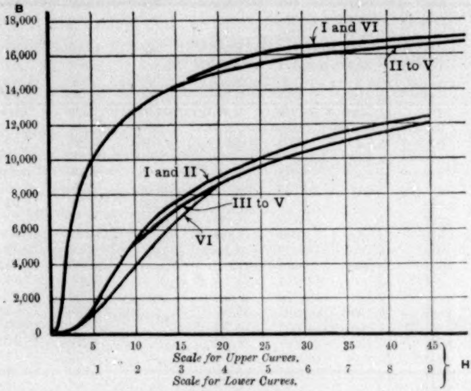

FIG. 5.—MAGNETISATION CURVES.

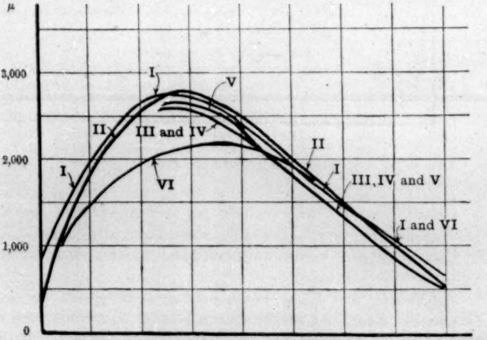

FIG. 6.—PERMEABILITY CURVES.

for the practical suggestions and assistance in the experimental work given by Mr. F. Stroude, B.Sc.

DISCUSSION OF RESULTS.

An examination of the divergences shown in Fig. 8 has led the authors to divide, for purposes of consideration, the curves into three parts: (1) From zero to the approximate point of maximum permeability (**B**=5,000); (2) from this point to **B**=15,000; (3) above **B**=15,000.

1. *Curves below* **B**=5,000.—These may be divided into two groups: (*a*) Those in which the magnetisation curve is determined with the flux always changing in the same sense, the iron having previously been completely demagnetised; and (*b*) those in which each reading is determined after the iron has been carried through a sufficiently large number of cycles of magnetising force to obtain constancy in the corresponding value of the flux-density.

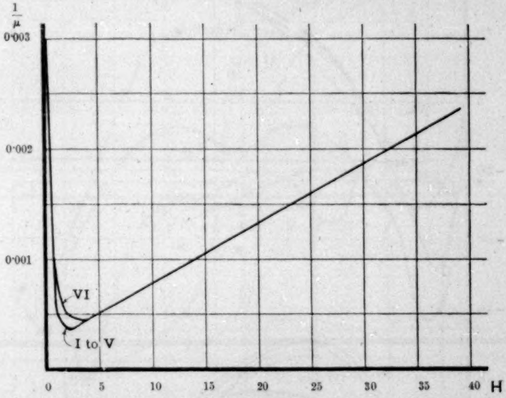

FIG. 7.—RELUCTIVITY CURVES.

Considering first those which belong to group (*a*), we have two cases: (i.) That of the magnetisation curve obtained by the uniformly varying flux method (method I.), and (ii.) the magnetisation curve determined by the step-by-step method (method III.).

It will be noted in Fig. 8 that there is a difference between these two curves of, broadly speaking, some 250 lines per square centimetre, from $\mathbf{B}=1,000$ to $\mathbf{B}=12,500$, and that the step-by-step method is deficient by this amount. This difference appears to be due to some flux not being recorded in the portion of the magnetisation curve below $\mathbf{B}=500$ in the step-by-step method. It is quite probable that this is due to the known behaviour* of iron under weak magnetising forces. When a magnetising force of, say, $\mathbf{H}=0{\cdot}2$ is suddenly applied, the flux at once changes by a certain amount and afterwards continues to change, though at a diminishing rate ; this latter " creep " would not be recorded by an ordinary ballistic galvanometer,

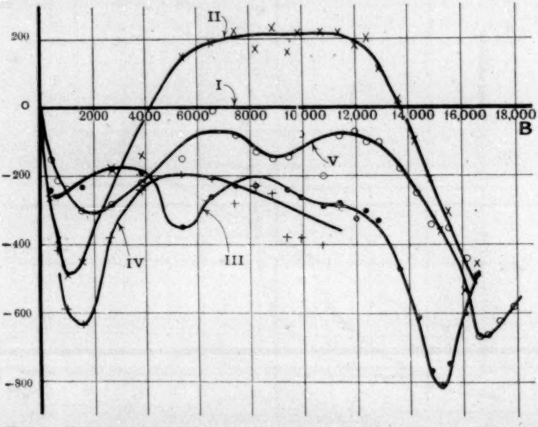

Fig. 8.—Curve showing Differences between Magnetisation Curves obtained by Various Methods.

and therefore would not wholly be taken account of in the step-by-step method, but it is included in method I.—that of uniformly varying flux. (The time of one complete swing of the ballistic galvanometer used throughout these experiments was 8·6 seconds.)

With the object of investigating this point the following experiment † was made. With the connections arranged as for the ballistic test, a sudden change was made in the magnetising current while the circuit of the galvanometer and secondary winding was held open, so that the galvanometer did not receive the " kick " due to the instantaneous change of flux in the ring. The galvanometer circuit was then immediately closed, so that if there were any residual creeping of the flux there would be a steady deflection of the galvanometer.

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* J. A. Ewing, " Magnetic Induction in Iron and Other Materials," p. 124.
† Hopkinson, " Original Papers," Vol. II., p. 257.

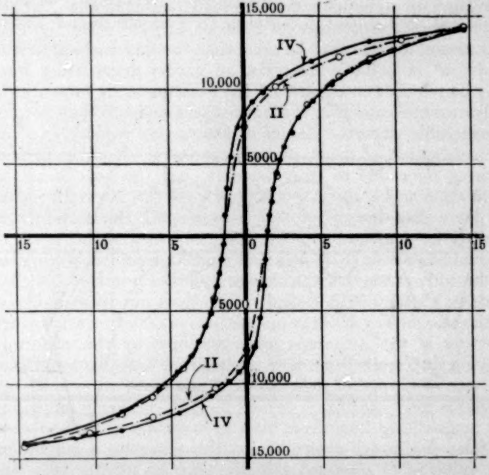

Fig. 9.—Hysteresis Loops by Methods II. and IV.

The maximum creep observed occurred when a magnetising force of $\mathsf{H}=1\cdot 25$ in one direction was suddenly changed to the value corresponding to the maximum instantaneous permeability in the other direction ($d\mathsf{B}/d\mathsf{H}=6{,}000$). Under these conditions, the creep was 40 lines per square centimetre per second, about 0·1 second after the change in H, and this creep diminished to 3·3 lines per square centimetre per second after about 1 second, disappearing entirely after about 5 seconds. On completely reversing the current the creep after about 0·1 second was only 4·6 lines per square centimetre per second, and after 1 second it had decreased to less than 1 line per square centimetre per second. This effect, therefore, although appreciable, does not appear to account wholly for the observed deficiency in the ballistic magnetisation curve. It will be noticed that all the curves in Fig. 8 start with a sharp downward inclination from the axis, showing that during the initial part of the magnetisation curve the highest value of $\mathbf{B}$ for a given value of $\mathbf{H}$ is attained by the method of uniformly varying flux, or, in other words, a higher value of $\mathbf{H}$ is required to produce the same observed value of $\mathbf{B}$ by the other methods. One reason for this may be that when the flux is changed suddenly, as in ballistic methods, an excess magnetising force may be required to counteract the demagnetising force due to eddy-currents caused in the laminations, and this may become considerable when the change of flux is very rapid. In order to investigate this point, an estimate was made of the demagnetising force due to eddy currents, and this was found by calculation to be about $0{\cdot}00000046 \times d\mathbf{B}/dt$. Now the excess $\mathbf{H}$, for a flux-density of 500, required for the step-by-step method, as compared with the uniformly varying flux method, amounts to about 0·15, and this amount would be accounted for by eddy currents if the change from $\mathbf{B}=0$ to $\mathbf{B}=500$ took place in about 0·0015 second, which does not seem unlikely. In the case, however, of the magnetisation curve drawn through the tops of the hysteresis loops obtained by the uniformly varying flux method another explanation must be sought, as the rate of change of flux, $d\mathbf{B}/dt$, is too small to produce appreciable eddy currents. It may be that, on slowly increasing the magnetising force from zero to a certain value and then carrying it through cycles having this value for a maximum, the value of $\mathbf{B}$ obtained initially may be greater than the value obtained after the iron has attained the cyclic state, owing to the first loops being unsymmetrical about the axis of $\mathbf{H}$.

With regard to group (*b*), which includes methods II., IV. and V., it is difficult to find any rational explanation for the differences observed. The authors have, however, been impressed with the idea that when a sudden change of considerable magnitude is made in the magnetising force the instantaneous flux variation, as compared with the total flux variation, may depend on the steepness of the magnetisation curve at the point attained instantaneously by the flux; in other words, it might depend not so much on the permeability at that point as on the rate of change of induction with magnetising force—*i.e.*, on dB/dH. One is led to imagine that if dB/dH is great, then the stability of the molecular magnet grouping is correspondingly weak, and, keeping the Ewing model in view, one would predict that, for a sudden large change in the magnetising force, a magnetic creep depending to a certain extent on this factor dB/dH would be expected.

This quantity dB/dH, and also the permeability, both calculated from the magnetisation curve obtained by the method of uniformly varying flux (method I.), have been plotted in Fig. 10.

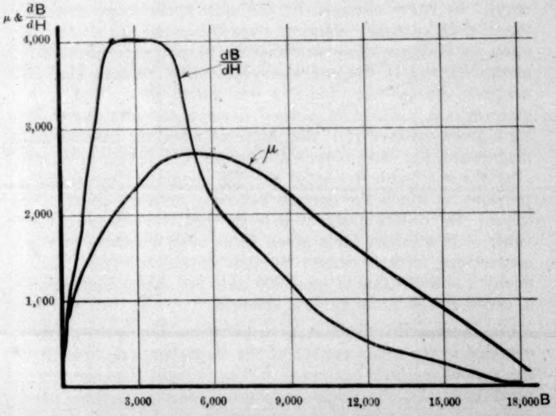

FIG. 10.—μ AND $\frac{dB}{dH}$ CURVES.

It will be observed from Fig. 8 that the maximum deficiency in B as obtained by methods II., IV. and V., occurs at about that part of the magnetisation curve where $B = 2,000$, and Fig. 10 indicates that this is the point where the steepest part of the magnetisation curve commences, or, in other words, where dB/dH attains its maximum value. It appears probable, therefore, that the maximum value of B attained when the iron is carried through a sufficiently large number of cycles of H depends to some extent upon the slope of the magnetisation curve at that point, **B** having a diminished value where the magnetisation curve is steepest.*

2. *Curves between* **B**=5,000 *and* **B**=15,000.—From Fig. 8 it will be observed that over this range, whilst the magnetisation curve obtained by the step-by-step method (method III.) maintains its deficit of about 200 lines per square centimetre with respect to the magnetisation curve obtained by the proposed standard method of uniformly varying flux (method I.), the magnetisation curves obtained by methods II., IV. and V., in which the iron is brought into a cyclic state before readings are taken, show a decreased deficit with respect to the standard curve, the curve obtained by the slow cyclic loops method (method II.) actually showing, over this range, an excess of some 200 lines per square centimetre. During experiments by methods I. and II. the iron was subjected to the same kind of magnetic change—*i.e.*, the flux was varied slowly and at a uniform rate. Whilst in method I. the observed value of **B** for a given value of **H** is that attained when the iron is first magnetised, the value observed by method II. is that attained after the iron has been carried through about six loops slowly, previous to which the current has been reversed about 100 times. The results appear to indicate that over this range the value of **B** attained for a given value of **H** increases as the magnetising force is carried through successive cycles, until finally a steady value of some 200 lines per square centimetre in excess of the initial value is attained.

Curves above **B**=15,000.—As considerable divergencies were obtained in the upper regions of the magnetisation curves by the various methods examined, it was thought that the temperature of the iron might have some bearing on these discrepancies, for in some of the experiments the full magnetising current may have caused a rise of 100°C. in the worst case. To investigate this matter the magnetisation curve of the iron was taken with the specimen at normal temperature and repeated with the ring heated by means of an alternating current to a temperature above that which it was likely to have attained in any of the foregoing experiments (some 150°C.).

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* To make this investigation more complete, it would be necessary to devote considerable time and attention to the experimental study of magnetic viscosity, and it was in the hope of being able to do this that the publication of this Paper has been delayed for nearly two years; leisure for this, however, has not been forthcoming, and consequently it is thought advisable to publish the investigation as it stands.

Results were obtained similar to those described by Dr. J. Hopkinson.* For small magnetising forces, say up to **H**=5, the permeability was increased, whilst for strong magnetising forces it was decreased, as a result of the rise of temperature. Portions of the magnetisation curves taken with the iron hot and cold are shown in Fig. 11. On the same diagram are plotted the points obtained as the maximum values reached in the " slow cyclic " hysteresis loops (method II.). It will be noticed that, as the limiting values of these loops increase, so they gradually pass from the " cold " to the " hot " curve. The authors, therefore, conclude that the results obtained above **B**=15,000 are of little scientific value.

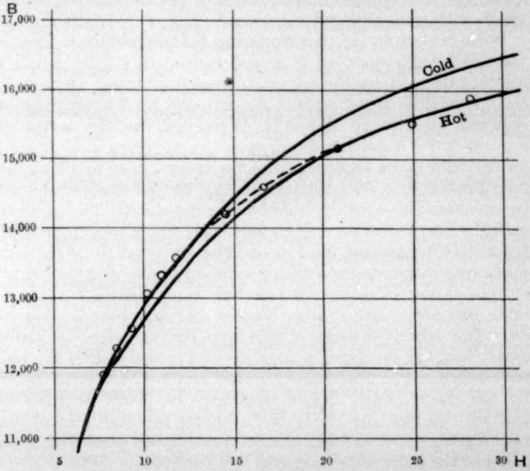

FIG. 11.—MAGNETISATION CURVES WITH IRON HOT AND COLD.

DEGREE OF ACCURACY.

In comparing the accuracy attainable by the various methods it is only necessary to consider the accuracy of the means employed to measure the flux-density **B**, the measurement of the magnetising force H resolving itself merely into a measurement of current, which may be made by the same means, whatever be the method used in determining the flux-density, except in the case of the alternating-current method.

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* Hopkinson, " Original Papers," Vol. II., p. 186.

Methods I. and II., Uniformly-varying Flux.—The measurement of flux-density by this method involves (1) the opposition of a constant P.D. to the E.M.F. induced in the secondary winding, and the measurement of this opposing P.D.; (2) the maintenance of equality between the generated and opposing voltages; and (3) the measurement of the time during which the flux is being changed.

With regard to (1), the opposing P.D. is obtained from a secondary cell connected to a high resistance, subdivided in a known ratio. Since the current given by the cell is very small, there is no difficulty in attaining constancy for the few minutes required for a test. The P.D. of the cell may be measured accurately by a potentiometer, and the opposing P.D. is then known with great accuracy from the known ratio in which the resistance is divided. As regards (2), the galvanometer used to ascertain the equality between the generated and opposing E.M.F.s may be as sensitive as desired. It is used merely as a "null" instrument, and its calibration curve is of no consequence. Since the secondary winding of the ring carries no current, it may be made of wire as thin as mechanical considerations will allow, and a large number of turns may be wound on. The rate of change of the flux may therefore be made small, without involving a very minute opposing E.M.F., and the change of flux represented by the smallest time measurable by the chronometric device used may be made as small as desired. In the case of (3), the accurate measurement of the time during which the flux changes presents no great difficulty, being of the order of from one to five minutes.

Methods III., IV. and V., Ballistic Methods.—The measurement of flux-density by these methods depends on the observing of the throw given by a ballistic galvanometer, the calibration curve of which has to be determined with the aid of a mutual inductance. The accuracy attainable depends, therefore, on the accuracy of the mutual inductance, the constancy of the zero of the galvanometer, and also to a lesser extent upon the time period of the moving system of the galvanometer (8·6 seconds), the accuracy being greater as the time of swing is greater, owing to the movement of the swinging system being less in the time during which the impulse is being received.

Method VI., Alternating-current Method.—The accuracy of this method depends upon the accuracy with which the R.M.S. value of an alternating current can be measured, and also upon the accuracy with which the wave-forms of current and flux can be determined. By the use of a thermo-galvanometer the R.M.S. value of the magnetising current and of the induced E.M.F. may be measured with great accuracy, but the measurement of oscillograms presents difficulties owing to thickness of the line traced out on the plate by the vibrating beam of light and other causes, whereby the percentage accuracy in the measurement of the form-factors is not so great as that attainable in the measurement of currents.

Observation Errors.—In Tables V. and VI. are set out the differences observed between curves taken on different dates by the same methods in order to give an idea of the accuracy of repetition. In Table VII. are given the collected results expressed in percentage accuracy.

TABLE V.—*Accuracy of Repetition of Magnetisation Curve by method of Uniformly Varying Flux.*

Flux-density **B** by standard curve taken December 14 and 16, 1908..................	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000	9,000
Difference in **B** between this curve and curve repeated by same method February 9, 1909..................	+32	+32	+20	0	−10	+40	+62	+15	+80

TABLE VI.—*Accuracy of Repetition of Magnetisation Curve by Ballistic Methods.*

Flux density **B** by method of uniformly varying flux (method I.)...	2,000	4,000	6,000	8,000	10,000	12,000	14,000	16,000
Difference in **B** between curves taken May 5, 1908, and April 30, 1909, both by "step-by-step" method (method III.)...............	−40	−230	−140	−55	0	−50	+30	−160
Difference in **B** between curves taken August 4, 1908, and April 20, 1909, both by "method of reversals" (method V.)	+125	+80	−30	+25	+180	−20	−220	−290

TABLE VII.—*Comparison of Percentage Accuracy.*

——	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000	9,000
Uniformly varying flux	+3·2	+1·6	+0·7	0·0	−0·2	+0·7	+0·9	+0·2	+0·9
Step-by-step	...	−2·0	...	−5·7	...	−2·3	...	−0·7	...
Reversals..................	...	+6·2	...	+2·0	...	−0·5	...	+0·3	...

Effect of Intervening Temperature Rises on Accuracy of Repetition.—The authors have, after considerable experience, had forced upon them the fact that if determinations of permeability upon the same sample are to be repeated with an accuracy of even 5 per cent. on the lower portions of the magnetisation curves it is essential that the ring be not subjected to considerable rises of temperature, say of 100°C., even for short periods. Effects due to heating have been noted by many experimenters, such as S. R. Roget, in a Royal Society Paper.* Rises of temperature may be brought about by demagnetisation with alternating currents, or by the heat produced by excessive currents used with the object of determining the induction density at high values of **H**. The discrepancies observed at low values of **H** between two determinations made at the same temperature, but at different times, with an intervening period of heating, are not to be confused with the previously mentioned discrepancies in the higher regions of the magnetisation curves which are due to the differing temperature of the iron whilst the test is being conducted. If the iron be allowed to regain its original temperature after being heated, tests may be repeated accurately in the higher regions of the magnetisation curve.

TIME REQUIRED FOR TESTS BY DIFFERENT METHODS.

As regards the time required for determinations by the various methods experimentally examined, ballistic methods are undoubtedly the most tedious, particularly when, for great accuracy, a ballistic galvanometer with a long period of swing is employed. The alternating-current method (method VI.) has considerable advantages in this respect, a full set of readings of magnetising current and induced voltage occupying a very short time. When, however, it is necessary to take oscillograms at various points in order to plot curves of form factors, the time required is enormously increased. The method of uniformly-varying flux (method I.) is peculiarly adapted for use where time is a consideration, and at the same time a high degree of accuracy is desired. A complete magnetisation curve may be taken in a very few minutes, and the mean of many such curves obtained in, say, one hour, the iron being demagnetised between each test.

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* S. R. Roget, "Effects of Prolonged Heating on the Magnetic Properties of Iron," THE ELECTRICIAN, Vol. XLI., 1898, p. 182.

CONCLUSION.

The method of "uniformly varying flux" appears to possess advantages, both scientific and practical, over the older methods in use for the testing of ring samples of magnetic materials. It avoids difficulties due to eddy currents and magnetic viscosity, which effects are themselves due primarily to rapid or irregular changes of flux. It also has the practical advantage that experiments may be carried out with rapidity, accuracy of repetition, and under standard or predetermined conditions of magnetic change.

The authors, therefore, would commend this method to the careful consideration of those interested in the carrying out of magnetic tests, especially where great accuracy under definitely known conditions of experiment are essential.

ABSTRACT.

The research described was instituted with the object of finding what differences there were between the magnetisation curves for a given sample of iron when determined (1) by the older methods in which the flux is changed suddenly, (2) by a method in which it is changed exceedingly slowly and at a uniform rate.

The methods experimentally examined were :—

1. Method of constant rate of change of flux.
2. "Slow cyclic" hysteresis loops by method 1.
3. "Step by step" magnetisation curve.
4. "Step by step" hysteresis loops.
5. Method of reversals.
6. Alternating-current magnetisation curve.

Details are given of the theory and practical working of method 1. In this method the magnetising current is continuously increased through a primary winding by a specially designed resistance at such a varying rate as to maintain a constant voltage generated in a secondary winding. A certain amount of skill is required in operating the resistance, but an average experimenter may easily acquire this with a little practice. The complete change of the current occupied times varying from one up to some five minutes.

Tables are given of the magnetisation curve determined by the six different methods at a number of values from H 0·3 up to 70·0, and of the permeability from B 500 up to 17,000.

At low values of the magnetising force the uniformly varying flux method gives results of some 200 lines per square centimetre in excess of the older methods.

As regards the time required for determinations by the various methods experimentally examined, ballistic methods are undoubtedly the most tedious. The alternating-current method (method 6) has considerable advantages in this respect, a full set of readings of magnetising current and induced voltage occupying a very short time. When, however, it is necessary to take oscillograms at various points in order to plot curves of form factors, the time required is enor-

mously increased. The method of uniformly-varying flux (method 1) is peculiarly adapted for use where time is a consideration, and at the same time a high degree of accuracy is desired. A complete magnetisation curve may be taken in a very few minutes, and the mean of many such curves obtained in, say, one hour, the iron being demagnetised between each test.

The method of "uniformly varying flux" appears to possess advantages, both scientific and practical, over the older methods in use for the testing of ring samples of magnetic materials. It avoids difficulties due to eddy currents and magnetic viscosity, which effects are themselves due primarily to rapid or irregular changes of flux. Besides rapidity of experiment it also has the advantage of accuracy of repetition under standard or predetermined conditions of magnetic change.

The method is, therefore, commended for the carrying out of magnetic tests, especially where great accuracy under definitely known conditions of experiment are essential.

DISCUSSION.

Mr. A. Campbell remarked that it was rather difficult to say whether it was tests made with a slowly varying flux that were required in practice. He thought tests made with cyclical changes were more useful. The differences in the results given by the two methods were important. He pointed out that the method could be used for the determination of the ohm by substituting a known mutual induction for the iron under test.

Dr. A. Russell pointed out that the value of H was different at the inner and outer radii of the iron ring used by the authors.

Dr. S. W. J. Smith said it would have been interesting and instructive if the authors had performed a similar series of experiments by the different methods upon some material freer from magnetic viscosity than soft iron. Experiments made upon a sample of nearly pure iron in weak fields did not seem to be explicable along the lines suggested by the authors for the iron they used.

Prof. C. H. Lees drew attention to the extraordinary simplicity of the reluctivity curves. It looked as if $1/\mu$ were a linear function of H.

The President remarked that by superposing on the constant magnetising field an alternating field which was gradually reduced to nothing, much larger values of the apparent permeability were obtained. For weak fields it was over 100,000. This effect was due to the residual magnetism left by the alternating field.

Prof. J. T. Morris, in reply, stated that they had not tried their method at widely different speeds. In reply to Dr. Russell, he stated that the same iron ring was tested by the different methods. Replying to Mr. Campbell, he had thought of the present method as a means of measuring a mutual induction.

XXIX. *Some Radio-telegraphic Apparatus in Use at the City and Guilds Engineering College.* *By Prof.* G. W. O. Howe.

Read March 24, 1911.

Although experiments in wireless telegraphy have formed part of the third-year course in electrical engineering for the last four years, it is only recently that attempts have been made to pick up signals from a considerable distance. By means of a new antenna of great height, and the receiving apparatus which is fitted up on the lecture table this evening we have succeeded in picking up the signals sent out from the Marconi station at Glace Bay, Nova Scotia.

The antenna employed for this work consists of a single wire of No. 14 copper-coated steel. The upper end is attached to the central tower of the Imperial Institute, 260 ft. above the ground. It is brought across in a single span of 530 ft. to a chimney stack on the college, from whence a wire passes down into the laboratory. For the purposes of the demonstration another wire has been connected to the antenna and brought into the lecture theatre. The fundamental wave-length of this antenna is about 900 metres.

In addition to this a T antenna is suspended horizontally above the roof of the college. It consists of five parallel wires 190 ft. long and 110 ft. high. A single wire connects the mid point to the laboratory. The fundamental wave-length is about 520 metres.

The receiving transformer or " jigger " used for short wave-lengths is on the principle of the Ayrton-Perry variable standard of self-induction, the primary coil being rotated within the fixed secondary in order to vary the coupling.

The wave-length of the electromagnetic waves sent out by the Marconi stations at Clifden, on the west coast of Ireland, and at Glace Bay is over 6 km. In the special transformer constructed for receiving these low-frequency waves, the coils are square, being supported at the corners on ebonite strips fitted to a wooden frame. The primary coil is movable, and can be raised 4 ft. above the fixed secondary by turning a handle. A lead weight below the transformer serves to counterbalance the weight of the moving coil. Each coil is subdivided into three parts, and the ends brought to a plug board. The wire consists of $3 \times 3 \times 3$—*i.e.*, 27 wires No. 36, each wire single silk covered, and the whole double silk covered over all. This ensures the high-frequency currents being distributed uniformly between the 27 wires. The primary has 160 turns 13·5 in. square, the winding length is 16 in., and the inductance of the coil is 6·45 millihenrys. The fixed secondary has 187 turns 15 in. square, the winding length is 13 in., and the inductance 12·9 millihenrys. The height over all when the coupling is a minimum is about 9 ft.

If the inductance, which must be placed in series with the aerial to tune it to various wave-lengths, be plotted against the square of the wave-length, the curve obtained is a straight line through the origin, except for short wave-lengths, where the inductance of the aerial itself is comparable with that placed in series with it. The slope of the line depends only on the capacity of the aerial, which, in the present case, is 1·35 milli-microfarads, so that an inductance of about 7·8 millihenrys must be placed in series with the aerial to tune it up to a wave-length of 6,200 metres. The exact inductance required for any wave-length is obtained by using suitable coils of the transformer primary and by adjusting the so-called tuning coils; but for shorter wave-lengths than 1,200 metres, capacity must be inserted in the aerial tuning condenser.

The secondary circuit consists of a part or all of the secondary coil of the transformer and a variable condenser. As arranged the apparatus has a range of 300 to 7,000 metres wave-length.

By calibrating the secondary oscillating circuit it can be used as a wave meter in determining the wave-length of the signals received. There are several sources of error, however, to be taken into account. The coupling must be sufficiently loose, as the tuning in the secondary circuit changes with the coupling when the latter is too tight. Another source of error is the detector, which in the arrangement used is shunted across the condenser. Switching over from one detector to another will sometimes cause an apparent change in the frequency of the received signal. If the variable capacity be small, an error is introduced if the self-capacity of the coil itself be neglected. If the coil acted as a simple inductance, and had no self-capacity, a curve connecting wave-length and $\sqrt{K}$ would be a straight line through the origin; but even when the capacity K of the variable condenser is reduced to zero the coil itself has a natural frequency of its own, so that for $K=0$ the wave-length is not zero, but has a perfectly definite value. When K is large the error introduced by neglecting this effect is negligibly small, but when K is small the error becomes very important. It is not only the effective capacity which is affected, but, owing to the current being less at the ends than at the middle of the coil, the effective inductance is less than the real inductance of the coil. This subject was investigated by Drude in 1902; he established empirical formulæ for calculating the natural frequency of coils of various shapes, but there is still room for further work on the subject.

The connections of the complete receiving apparatus are shown in Fig. 1. There are two alternative detectors; those actually used for the demonstration are exactly similar, consisting of a light contact between small pieces of zincite and copper pyrites. This is very sensitive, but is rather troublesome, as it needs frequent adjustment as a rule, and is apt to be rendered inoperative by a bad "atmospheric." The Glace Bay signals can only be heard at night, and the necessary coupling is rather tight; this makes the atmospheric disturbances very troublesome, it being sometimes impossible to hear the signals through them for several seconds. Darkness favours the transmission of electromagnetic waves,

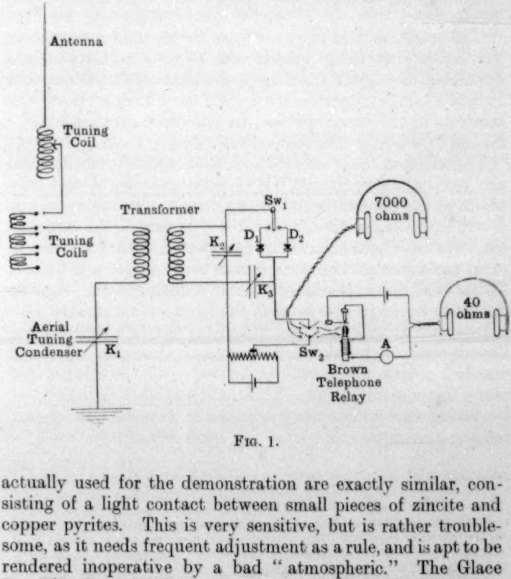
Antenna
Tuning Coil
Transformer
Sw_1
7000 ohms
D_1
D_2
K_2
Tuning Coils
K_3
40 ohms
Aerial Tuning Condenser
K_1
A
Sw_2
Brown Telephone Relay

FIG. 1.

irrespective of their source, and, as lightning flashes are probably much more powerful than the waves sent out by a radio-telegraph station, one may very well hear every lightning flash on the globe as a crack in the telephone. The disturbances often resemble the rattle of musketry. This is undoubtedly the greatest drawback to long-distance radio-telegraphy.

It is evident that the most sensitive detector is not necessarily the most advantageous. One must sacrifice sensibility to obtain reliability. The relative sensibility and convenience of different types of detectors is a matter on which one should hesitate before giving any dogmatic opinion, on account of the different conditions under which each type gives its best results.

The detectors used at the college are on the lecture table; they include the filings coherer with inker, &c. (the standard apparatus five or six years ago), the electrolytic with various sizes of wire, the magnetic detector and the Fleming valve. The minerals in the detectors used in the demonstration can be replaced by others in a few seconds.

The telephone head set used with the zincite-pyrites detector has 7,000 ohms resistance. By means of a switch this telephone can be cut out and its place taken by a Brown telephone relay* operating a 40-ohm head set, in which the ordinary earpieces have been replaced by trumpets. With this arrangement the messages and time signals sent out from the Eiffel Tower and from Norddeich, near Wilhelmshaven, can be distinctly heard by every one in the large lecture theatre.

It is only by using the telephone relay that the signals from Canada can be heard; unfortunately, the relay does not manifest a wise discrimination between radio-telegrams and atmospheric disturbances. This could probably be improved by tuning the reed to the pitch of the musical note of the sending apparatus—that is, to the spark frequency.

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* *See* "Trans." of Inst. of Elec. Engineers, Vol. XLV., p. 590.

XXX. *The Lüders' Lines on Mild Steel.* By W. MASON, *M.Sc.*, *The University, Liverpool.*

FIRST RECEIVED MARCH 29, 1911. RECEIVED IN REVISED FORM MAY 26, 1911.

AT the time of elastic breakdown under stress of all the ductile metals, slipping occurs along surfaces of weakness in the crystals. Such slips are apparent as " slip-lines " or " slip-bands," when microscopic examination is made of surfaces of the specimens which have been polished and etched before straining.[*] Moreover, on the surface of iron and steel specimens there become visible, at or about the yield point, lines or bands, known as " Lüders' " or " Hartmann's " lines.[†] These lines exhibit themselves on the large scale, commonly extending over a large extent of the surface of the strained specimen. Somewhat similar lines have been observed by Hartmann and others on the surfaces of many other ductile metals, but there seems to be some doubt whether these are of the same kind as those on iron and soft steel.[‡] The writer has confined his attention to the last-mentioned material, and it has been his object to collect additional information concerning the occurrence of these lines upon mild steel, to determine the angular incidence of these lines, and also, if possible, the angles of the corresponding internal surfaces with respect to the stress directions, in specimens loaded in various ways.

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[*] Ewing and Rosenhain, " Phil. Trans " A, 1900, Vol. 195.

[†] Hartmann. " Distribution des déformations dans les métaux soumis a des efforts," 1896, Paris, Berger-Levrault et Cie.

G. H. Gulliver. " Proc." Inst. Mech. Engineers, No. 1, 1905,

G. H. Gulliver. " Proc." Roy. Soc., Edinburgh, Vol. XXX., Part I.

G. H. Gulliver. Congrès International des Méthodes d'Essai, 1909.

Fremont. " Bulletin " de la Société d'Encouragement, Sept. 1903 ; or " Nature," Jan. 21, 1904.

Also *see* Congrès International des Méthodes d'Essai, 1900, I., 95 ; 1900, I., 143 ; 1900, I., 185.

[‡] G. H. Gulliver. International Association for Testing Materials, 1909, VIII., 9, pages 2 and 3 of translation into English. "From the results of his (Mr. Gulliver's) experiments on most of the metals and alloys on which Hartmann and others claim to have observed the lines of Lüders', Mr. Gulliver is of opinion that what these investigators really obtained were contractile depressions. So far as Mr. Gulliver's experience goes, the lines of Lüders' are only found on iron and steel—metals which have a well-marked yield point." The author has recently tested some drawn lead tubes, of various wall thicknesses, under end pressure and also under simultaneous end pressure and internal fluid pressure. No lines were observed. Lead is one of the metals on which the lines were said to have been obtained. The lead tubes were in the author's possession for four months before being tested—an item not unimportant in view of the phenomenon of recrystalization in lead observed by Ewing and Humphrey.

On mild steel specimens in tension these lines occur on angles not differing much from 50 deg. to the direction of pull; and although, so far as the writer is aware, no definite information up to the present is obtainable concerning the direction of the corresponding internal surfaces, it seems probable that the latter are inclined at the same angle, viz., about 50 deg.

It is remarkable that these lines should be so regular and uniform in angle on iron and steel specimens in tension. They are no doubt mainly due, like the crystalline cleavage slips of Ewing and Rosenhain, to shear stress. In a tension specimen, however, the planes of maximum shear stress are any ones of a family all at 45 deg. to the direction of pull; and, moreover, the orientation of the crystals, from grain to grain, is entirely random. Under such conditions, it would not be expected that any continuous series of grains in which slipping has occurred in the same direction would be possible, much less any continuous surfaces or lines of slipping. The phenomenon is one very intimately related to that of the yield point, the latter, if not confined entirely to iron and steel, being absent or only partially exhibited by other metals.

On mild steel specimens in compression the Lüders' lines do not appear, from the published work, to be so regular in inclination as for tension. Mr. G. H. Gulliver finds that the angle lies usually between 40 deg. and 45 deg. and sometimes " even above 45 deg." to the direction of the compression.* The writer finds, in tube specimens of mild steel, that the angle is always greater than 45 deg., and is about 50 deg., the same as for tension. (*See* Tables A, B, C, D.) (The lines at 90 deg. are apparent exceptions only; these are unmistakably traces of conical surfaces—the axis of the cone being coincident with the axis of the tube.) He has found, too, when tensile and compressive stresses of equal intensity are applied simultaneously in directions at right angles to each other that the lines are at 45 deg. to these directions, or thereabouts.†

Occurring as these lines do at or about the yield point of the mild steel, a study of them is likely to throw light on the connection between the process of yielding and the stresses causing it.* The writer decided to continue to use tubular specimens because of the difficulty of applying (sensibly) uniform stresses, except by their use, in two directions at right angles. It was easy, too, to subject a tube of fairly large diameter and small thickness to longitudinal compression as well as to a small amount of hoop tension, by internal fluid pressure; and by so doing to give the planes of maximum shear stress two determinate directions only. This done, it seemed probable that the directions of slipping would be more regular and uniform than they are when longitudinal compression only is applied, and when, therefore, *all* planes at 45 deg. to the axis of the tube are planes of maximum shear.

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* " Proc." Roy. Soc., Edinburgh, Vol. XXIX., Part V., No. 28, page 439.

† " Proc." Inst. Mech. Eng., Vol. IV., 1909.

The use of tubes, too, facilitated the reversal of the stresses, *i.e.*, the making of tests with the tubes in longitudinal tension and hoop compression, as well as with end pressure and hoop tension.

Scheme of the Tests.

Four series of tests were made; three series (Tables A, B and C) were made on mild steel tubes subjected to longitudinal compressive stress simultaneously with the application of interior hydrostatic pressure; and one series (Table D), in which some of the specimens were stressed as just mentioned, while others were made to undergo longitudinal tension simultaneously with water pressure applied externally.

In each series the loads and the water pressure were applied in such proportions that a set of tests was obtained with various ratios of longitudinal stress (p_1) to hoop stress (p_2).

The stress at the point of yielding of each specimen was observed, and the angle of inclination of the spiral lines of Lüders measured.

The Test Specimens.

Those of Tables A, B and C were cut from solid drawn steel tubing of three sizes, viz.,

14	L.S.W.G.	(about 0·08 in.	thickness),	bore 3 in.
11	,,	(about 0·118 in.	,,),	bore 2·5 in.
10	,,	(about 0·127 in.	,,),	bore 2·75 in.

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* Lüders' lines are not frequently observed in the usual course of testing. The writer has tested, at various times, several hundreds of bars and, excepting the specimens of this work, has observed the lines on very few of them. The most favourable condition for good development of them appears to be when one dimension of the specimen is small, *e.g.*, on flat bars or tubes. The lines were absent when tubes were tested thus:—

Simultaneous longitudinal tension and hoop tension of equal intensities.

Simultaneous longitudinal compression and hoop compression of equal intensities.

The 14 and 11 G tubing was cold drawn, while the 10 G was hot drawn. All the specimens (except those stated otherwise) were well annealed; and the ends were faced up in the lathe so as to be perpendicular to the length. Only perfectly straight specimens were tested.

It was impossible to obtain tubes of perfectly uniform thickness of wall. In nearly all the tubes the bore was slightly eccentric to the exterior surface; this eccentricity varied from 0 to 0·0035 in. in the 14 G tubes, and from 0 to 0·005 in. in the 10 G. The conical cap* through which the compressive load was applied was set on the tube so that the axis of the cone coincided with the centre of gravity of the metallic section of the tube, thus giving uniformity of compressive stress p_1. In no experiment was the eccentricity between the axis of the cone and the axis of the tube greater than 0·04 in. The hoop tensile stress, p_2, was calculated for the locality of least thickness. All the tubes were accurately calipered.

The specimens of Series D were tubes turned and bored out of hot drawn mild steel tube of bore $2\frac{3}{4}$ in. and thickness $\frac{1}{4}$ in. The finished bore was 2·86 in., and the thicknesses about 0·10 in. and 0·09 in. The thicknesses were accurately calipered. The specimens were placed in a sheath and heated in an electric furnace to a temperature 1,400°F. to 1,430°F. They were brought up to that temperature and allowed to remain thereat for 15 minutes; then they were quickly removed and allowed to cool in the sheath, the latter being placed in a covered drain pipe. The total time in the furnace was 45 to 50 minutes. The heat was controlled by a resistance coil which regulated the current, and a record was taken of the temperature of each specimen (or rather of the sheath) by a pyrometer. The main object of the treatment was not to anneal the tube, but to cover the bright turned surfaces with a thin film of scale; at the same time it was possible to treat all the specimens alike.

On all specimens the Lüders' lines were manifested by the disturbance of the scale. That the scale served as an indicator of the disturbance beneath itself, and that the lines were not merely due to the yielding of the scale under its own stressing, was evident from the extensometer indications. Evidence of this was apparent on the specimens themselves.

Apparatus for Longitudinal Compression and Hoop Tension.

The compressive loads were applied in a 100-ton Buckton vertical testing machine. In order to obtain axial loading, the load was applied through a conical cap, as described elsewhere.* Water was pumped into the interior of the tube through a hole in the base-block on which the tube sat; in all cases, except in certain of the specimens of Table D, water-tightness was obtained by thin load rings inserted between the ends of the tubes and the cone and base-block.

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* *See* Fig. 3, Plate 30, "Proc." Inst. Mech. Eng., 1909, No. 4.

When the pressure of water inside the tube necessary to obtain a given hoop tension was large, and when simultaneously with it a small longitudinal pressure only was required, the lead joint did not prove effective. Water-tightness was then secured by cup-leathers at the ends of the specimen.

In several of the tests of Series D, cup-leathers at the ends as well as lead rings were used in order that nearly the whole of the hoop tensile stress might first be imposed with no end pressure and therefore no frictional grip on the ends. The proper load for the required longitudinal compressive stress was then imposed.

The net load compressing a tube was found by subtracting from the load on the testing machine the upward thrust due to the water pressure (if any) on the area of the bore of the tube. The hoop tensile stress due to the water pressure was calculated regarding the tube as a thick cylinder, the stress p_2 in the Tables being that at the inside skin of the tube. A Ewing extensometer was used to observe the strain. It was not fixed directly by its own screw points to the tubes, which were too large in diameter for the instrument, but was fixed by means of a special detachment described elsewhere.*

Apparatus for External Pressure.

The external pressure, giving hoop compressive stress, was applied to the tubes by means of the jacket represented in Fig. 1. The tube to be tested passed through the cast-iron jacket J, whose ends are fitted with brass annular caps C_1, C_2 surrounding the tube. C_2 fits the tube loosely, while the opening in C_1 is large enough to admit the sleeve S, and to leave a space of about 1/50 in. between S and itself. The surfaces of S and C_1, forming the boundary of one end of the jacket space, are curved to fit a cup-leather L, which makes a water-tight joint between the tube and S, and between S and C_1.

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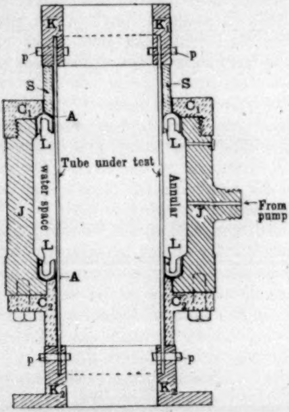
* "Proc." Inst. Mech. Eng., 1909, No. 4.

The other end of the jacket is made tight by another cup-leather L. The sleeve S and the lower end of C_2 bear against the holders K_1, K_2 which are fastened to the ends of the tube by pins *p*, *p*, which are equally spaced round the circumference. Under the pressure in the jacket, the sleeve S and C_2 (to which the main body of the jacket is bolted) tend to separate under a thrust which, being transmitted to the holders K_1, K_2, puts the tube specimen into tension. The amount of this tensile load has been taken to be: Water pressure × (sectional area of sleeve $+\frac{1}{2}$ annular area between S and C_1). This amount is a little doubtful on account of the rigidity of the leather covering the 1/50 in. annular space between S and C_1, and has been marked as uncertain in Table D.



Scale: $\frac{1}{2}$ Full Size.

FIG. 1.—JACKET USED FOR THE APPLICATION OF EXTERNAL PRESSURE TO THE TUBES.

Showing the arrangement used for the tests of Specimens Nos. 5, 6, 7 of Table D. Specimen No. 4 of Table D ($4\frac{1}{2}$ in. long) was placed in position with its ends at the points A.A.

In the test No. 4, Table D, the tube specimen only extended to the commencement of the curve of the cup-leathers, and separation between the jacket and S was prevented by large washers kept in position by a bolt passing axially along the interior of the tube.

Gauges and their Calibration.

The gauge used to indicate the water pressure was one of the Bourdon pattern. It was calibrated up to 400 lb. per sq. in. against dead weights. From this pressure upwards calibration was performed by means of an apparatus which, set up in the testing machine, weighed the hydrostatic pressure against the load on the machine. This apparatus was checked by a standard Bourdon gauge reading up to 3,000 lb. per sq. in., which was tested at the maker's works against dead weights.

The testing machine was calibrated by the Board of Trade about the time of the experiments.

Tests in Longitudinal Compression.

All tests in compression made on solid specimens whose length is greater than two or three times the smallest lateral dimensions are subject to the difficulty of instability, with consequent bending. In the case of tubes, this bending may be "column bending" after the manner of long solid struts, or may be bending or buckling of the tube walls. The latter manner of failure results in a sort of wave formation,* and the tube finally crushes down by the folding together of the crests and hollows of the waves. When the walls are very thin the wave formation will develop early in the test, and will cause failure before the elastic limit or yield point of the material in compression is reached.

This manner of failure has been investigated by Mr. A. Mallock,† who has calculated the inclination of the salient angles of the incipient spiral folds. These lines of spiral folding do not seem to have any direct relation to the Lüders' lines. They do not agree in inclination with the latter. And the lines on the tubes do not seem, from their appearance, to be caused by bending; many of the lines and bands are very precise and cleanly marked; also in the long lines there is nothing to indicate any periodic change along the length such as would be expected if they were lines of incipient folding. Quite as frequently as otherwise the lines were spirals of one hand only. There was, too, always a difference of position, lengthwise to the tube, between a "pair" of lines running circumferentially (*see* page 316), this difference between the inside line and the outside line of the "pair" being about equal to the wall thickness; this appears to be inconsistent with the assignment of folding

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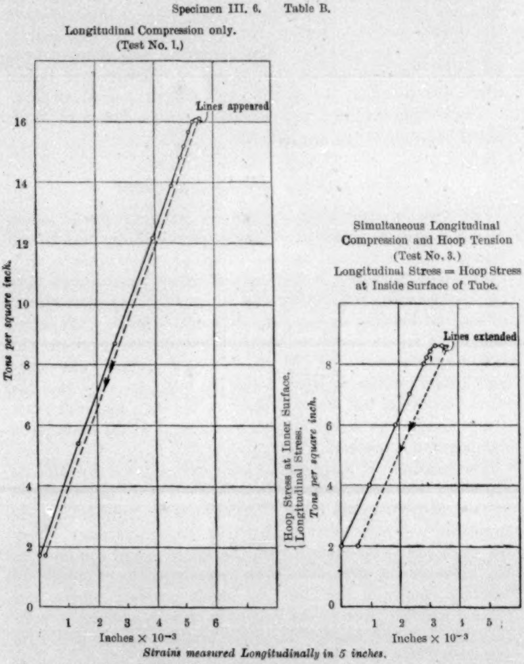
* "Secondary Flexure of Tubes." W. E. Lilley, "Engineering," Jan. 10, 1908.

† "Note on the Instability of Tubes subjected to End Pressure." "Proc." Roy. Soc., A., Vol. 81, No. 549. A. Mallock, F.R.S.



NOTE.—A curve test for No. 2 is not plotted, since the ratio between longitudinal stress and hoop stress was not kept constant during the test.

FIG. 2A. FIG. 2B.

STRESS STRAIN DIAGRAM ILLUSTRATING THE TIME OF APPEARANCE OF THE LÜDERS' LINES.

as a cause. There is, in fact, the closest resemblance between the lines on the tubes and the Lüders' lines on flat bars strained in tension.

Fig. 2A is a typical stress-strain curve; it illustrates the sharp bend at the yield point which most of the compression specimens exhibit. After a test up to the yield, the wall of a tube showed slight disturbance; but, except in one or two cases, no trace of flexure was observable before the yield and the appearance of the lines. An increase of about 3 and 8 tons per sq. in. was required to produce collapse, by the wave formation, in the 14 G and 10 G tubes respectively. With the ratio of thickness to diameter of the tubing used in the experiments, it seems probable that the yield point of the steel was reached before the commencement of flexure of the walls; and, at any rate, before any such flexure had material effect in causing failure.

Tests in Simultaneous Longitudinal Compression and Hoop Tension.

The yield point was sudden and very well defined.

Fig. 2B is a typical stress-strain curve. The increments of load and of internal pressure were arranged so as to give equal increments of compressive stress and equal increments of hoop tensile stress. At intervals during the increase of stress of the specimens of Tables C and D, the load on the testing machine was run back while the internal pressure was kept constant, until leakage of water just commenced. This was done in order to minimise frictional gripping of the tube ends. The length on which the strains were measured did not include about 1½ in. at each end of the specimen. No doubt there would be some gripping of the tube ends by the cap and pedestal and consequently some prevention of the lateral elastic expansion under the hydrostatic pressure. This effect would be diminished by the presence of the lead rings. At the compressive stresses corresponding to the yield points, the lead was always at its "pressure of fluidity." A set of three tubes of 14 gauge thickness, 3 in. bore, and 3, 6 and 18 in. long, when tested under such conditions that the compressive stress was equal to the hoop tensile stress, gave respectively for the shear stresses at the appearance of the lines 11·05, 11·25 and 11·10 tons per sq. in. If such stressing was continued beyond the yield point, the effect of the frictional gripping became very apparent; the tube became slightly barrel-shaped at the ends.

Tests in Hoop Compression with and without Longitudinal Tension.

The extensometer was fixed by means of its special attachment to the parts of the tube projecting beyond each end of the jacket (*see* p. 310). The extensometer was only intended to indicate the yield point, and not to give quantitative measurement of the strain. It was found, however, that the tubes collapsed before any decided yield point was indicated by the extensometer, showing that the means of measurement was not sensitive enough. The extensometer was not used on specimen No. 4 of Table D; instead, a strong inner tube about $\frac{1}{100}$ in. less than the bore of the test specimen was used as a feeling piece. When this became tight the test was stopped. This inner tube also prevented the complete collapse of the specimen. An inner tube, preventing collapse, was also used for specimens Nos. 5 and 6 of Table D.

The stresses due to the external pressure are marked as uncertain in the Table D on account of the aid given by the projecting ends (beyond the jacket) to the central part under pressure in the jacket. That this aid was considerable was evident from the high pressures necessary to cause collapse. The Lüders' lines were, however, very well defined, even on the specimens which collapsed. The planes of maximum shear were, in all the specimens, perpendicular to the plane of p_1 and p_2, and 45 deg. to a generator.

Mode of Occurrence of the Luders' Lines.

In the experiments of this Paper, the first appearance of "creep," or "time effect," observable on the extensometer was very quickly followed, with only an interval of stress of 0·1 to 0·3 ton per square inch, by the large increment of strain indicating the yield point. The Lüders' lines appeared sometimes at the first indication of "creep" and sometimes at a stress interval of about the above amount afterwards.* When once a line or lines had appeared (usually at an end of the specimen) these extended at either constant value of the imposed loading or with this only very slightly increased. This extension was usually accompanied by the broadening of the part of the line which first appeared. In nearly all cases the broadening took place equally on each side of the line, and in those cases where the spreading was unequal on the two sides, the inequality appeared to be due to the presence of other adjacent lines. When a line or lines had extended so far that the slope could be well measured, the test was, as a rule, stopped. In all cases the loading and water pressure were increased very slowly when the neighbourhood of the yield was reached; but occasionally the spreading and broadening was so rapid that the whole, or nearly the whole of the surface was so much affected that the inclinations of the lines could not be measured. While the name "lines" has been used—the name commonly given to them being "Lüders'" or "Hartmann's lines"—it would be more accurate to describe many of them as bands. In some cases the original line can be very clearly seen in the centre of a broadened band. On some specimens the lines formed one system of parallel or nearly parallel spirals (*see* photograph, Fig. 3); on others there were two systems of parallel spirals of opposite hand. In a few cases there were, in the neighbourhood of the intersection of two bands, a number of other very short lines apparently running in random fashion at diverse angles; apparently these lines were produced subsequently to the main long lines, and were probably due to the irregular conditions caused by the disturbance due to the first two lines. The lines on any one specimen were not strictly parallel to each other; in Tables A, B, C and D the maximum and minimum slopes are given as well as the average.

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* It was not possible to observe the time of the appearance of the lines on the tubes tested under water pressure external to the tube, nor of the lines on the inside surface on any of the specimens.

The formation of the Lüders' surfaces does not seem to be a process of massive slipping. In some cases when the stress, at the first indication of yield, was increased very slowly and carefully, the lines could be seen slowly extending themselves in their spiral way round the tube. To watch the development of the lines or bands gave the impression of the propagation of a disturbance rather than of a process of massive slipping.

Measurement of the Angles of the Lines.

The angles of the lines were measured by wrapping paper on the tube. It was very difficult to measure the spiral angles of the inside lines; on account of this difficulty the angles of only a few are given in the Tables. Sometimes there were no lines at all on the inner surface; probably this may be accounted for by the difference in the kind of scale on the inside and outside. Again, occasionally no lines showed on the outside when the extensometer indicated the neighbourhood of the yield point, but were to be seen on the interior surface after the test.

Specimens on which the lines were not sufficiently distinct for the angle to be measured were rejected unless the stresses at yield were useful for comparison. A number of specimens showed no lines. A troublesome circumstance was that, although the lines on a few of the 11 G specimens were vividly distinct at the time of the test. yet after the lapse of a day they were scarcely visible.

In several cases, after a test with a given ratio of p_1 to p_2 had been made, and the lines had appeared, a test with another ratio of p_1 to p_2 was made, and a new yield point sought, on the same specimen. In the second test, the lines which had previously appeared broadened out and extended their length, but usually the new extension was at a different spiral angle. Sometimes entirely new lines would appear in the second test; these were always at a different inclination to those of the first test.

On account of the close and uniform agreement between the time of appearance of the lines and the commencement of the yield as shown by the extensometer, the author did not think it necesssary to use the extensometer always, and accordingly many of the tests were made without the aid of that instrument.

One of the most interesting facts about the Lüders' lines was the occurrence of lines on the outside and the inside of a specimen, radially opposite, or very nearly opposite, to each other—*i.e.*, a radial line through the axis of the tube passed through both the inner and outer lines. This pairing of lines occurred on most of the specimens. The ends of a pair of pairs were also radially opposite.

That such "pairs" of lines or of bands were radially opposite was proved by cutting transversely a few of the tubes on which the lines were well marked on both surfaces, and scribing radial lines on the surface of the section. Another method was that of taking a point on an outside line and finding a point radially opposite to it on the inside surface by means of the scribing block and angle square. While it was not an easy matter to find the exact point where the centre line of a band or line intersected the edge of a cut section of tube, it was quite evident that the "circumferential displacement," if any, between an inner and outer line was quite small. Most of the lines appeared to be exactly opposite; there was apparently a small displacement in the case of a few lines, but only in three or four pairs of lines was there more displacement than could be accounted by error of measurement. It seemed that, most probably, local irregularity was the cause of the apparent displacement in these three or four cases. The operations for testing the radial positions of the lines were done with the greatest care, since upon them depended the determination of the planes or surfaces of yielding.

Determination of the Inclination of the Surfaces or Canals of Yielding from the Lüders' Lines.

Since pairs of lines, radially opposite on the tube surfaces, were always found (when, as in many specimens, there were lines on both surfaces) it is evident that a pair of lines or bands so situated must be the traces of the same spiral surface or canal. The absence of "circumferential displacement" between a pair of lines together with the fact that the ends of a pair were also opposite to each other indicate that a plastic movement, commencing probably at the inner surface, first makes its way through the tube wall, then extends in a spiral direction occupying in its advance the whole thickness of the tube wall. An inside line does not appear, therefore, to precede an outer line except at the commencement; and the process of spreading of the disturbance will not be, as would naturally be expected, a successive yielding from the inner surface outwards. If the process of yielding consisted in the penetration through the wall thickness of long lines first formed on the inner surface (where initially the stress is a maximum) the inclination of the lines on both surfaces would be the same.

Since this is not the case, the lines being radially opposite, the inclination of inside and outside surface lines are different; and measurements showed this difference to be that of two spiral lines, one on either surface, which make a complete revolution of the tube in the same axial length. Moreover, being radially opposite, a pair of lines must be the traces of a spiral surface generated by the movement of a radial line which always intersects a helix. In the appendix to this Paper the case of a plane skew to the wall of a 10 G tube is considered, the wall, however, being supposed plane instead of cylindrical. If the traces of such a plane be inclined at an angle a (not differing from 45 deg. by more than ±5 deg.), it is shown that there will be a "displacement" from the normally opposite position, which will be unmistakably measurable if the inclination of the skew plane itself differ as little as 0·75 deg. from a.[*]

Applying this to the cylindrical 10 G tube, it is evident that the Lüders' surfaces may be taken to be, without serious error, right spiral surfaces.

In view of the process of formation of the Lüders' surfaces, and also of the foregoing considerations, it appears to the author that, for the purpose of comparison of the inclinations of the surfaces in tubes of different thicknesses, the inclination at the mid-wall of the tube should be taken, rather than the angles of the traces. By doing this, the angles for the tubes of different thicknesses agree very fairly. Moreover, it seems reasonable to consider the mid-wall angle, rather than the angle on the inner surface as being the probable inclination of the Lüders' surfaces had not the circular form of the wall caused a variation from surface to surface. Accordingly, in the Tables A, B, C and D, the angles of the Lüders' lines, measured on the surfaces, have been reduced to the corresponding angle at the mid-wall by adding or subtracting to a surface angle half the difference of inclination between that angle and that of another spiral line radially opposite on the other surface.

On most of the tubes tested in simple compression there were lines or bands running circumferentially. These lines were not so long or so regular as the inclined lines, and the scale usually flaked somewhat on either side, making a ragged line. There were often lines, corresponding to them in position, or the internal surface; a pair of such lines were evidently the traces of a surface of yielding. There was always a longitudinal displacement between the inner and outer of a pair of these lines, the displacement being about equal to the wall thickness. But the lines were too ragged to obtain an accurate measurement of the displacement, and therefore the angle of the conical surface of yielding could not be obtained.

State of Stress during the Propagation of the Lüders' Lines.

Before any non-elastic strain has taken place the hoop stress will vary from a maximum at the inner surface of a tube to a

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[†] *See* remark in the Appendix, p. 324, concerning the thinner tubes.

minimum at the outer, this stress at radius r being given by the formulæ :—

For internal pressure p_i,

$$p_r = -\frac{p_i \cdot r_i^2\left(1+\frac{r_e^2}{r^2}\right)}{r_e^2-r_i^2},$$

For external pressure p_e

$$p_r = \frac{p_e r_e^2\left(1+\frac{r_i^2}{r^2}\right)}{r_e^2-r_i^2},$$

where p_r is the hoop stress at radius r,
r_i is the internal radius,
r_e is the external radius.

Thus, for internal pressure the hoop stress varies from $p_i\frac{r_i^2+r_e^2}{r_e^2-r_i^2}$ at the inner surface to $p_i\frac{2r_i^2}{r_e^2-r_i^2}$ at the outer surface; a variation of about 10 per cent. in the thickest tubes and about 5 per cent. in the thinnest tubes.

The inner surface is also the locality of greatest shear stress, this being half the difference between the hoop stress and the longitudinal stress. The first breakdown of elasticity will no doubt occur at some point or points of the inner surface, but at a very much lower stress than that at the yield point. Mons. F. Osmond * has remarked " that the stress-value of the elastic limit depended in crystalline bodies upon the crystalline orientation in comparison with the direction of the stress " ; thus ferrite crystals disadvantageously situated with respect to the stress direction should suffer elastic breakdown first. Also, considered microscopically, the structure of mild steel is heterogeneous, the weaker ferrite crystals being mingled with the harder pearlite A conclusion reached by Mr. L. Bairstow from his experiments on cyclical variations of stress is that there is elastic breakdown under a static stress very much less than the yield point stress, though the aggregate amount of the strain is too small to be detected by the extensometer.†

As the number and probably the extent of the localities of plastic strain‡ increase with increment of stress, this strain

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* "Proc." Inst. Mech. Eng., Vol. IV., 1909, p. 1,303.

† "Iron and Steel under Cylical Variations of Stress."—National Phys. Lab. "Collected Researches," Vol. VI., 1910, p. 181.

‡ Dr. Rosenhain was unable to find by microscopic examination evidence of any other kind of non-elastic strain other than that of cleavage slipping.—"Journal," Iron and Steel Institute, Vol. LXX., 1906.

will begin to be registered by the extensometer; and the stress-strain curve will show signs of departure from the straight line. The point of departure—if point it can be called, since its position is indefinite—is what is usually regarded as the "elastic limit." Between this "elastic limit" and the yield point quite a large stress difference is sometimes found; but in tests wherein care is taken to obtain uniformity of applied stress this difference is small—as was the case in nearly all the author's tests. The opinion has been expressed by some writers * that could perfectly uniform stress be imposed on a homogeneous test specimen of mild steel, the elastic limit and the yield point would be found to coincide. In view of the nature of mild steel and wrought iron the author ventures to think that the proximity of the extensometer "elastic limit" and the yield point must be due to some other cause than uniform stress distribution and homogeneity of the material.

In the author's tests of tubes the sharp bend in the stress-strain diagram—*i.e.*, the yield point—coincided with the appearance and spreading of the Lüders' lines. A noticeable circumstance, also, was the small aggregate amount of non-elastic strain indicated by the extensometer (Figs. 2*a*, 2*b*) when the lines appeared, but the comparatively large augmentation as the lines extended. Evidently the process of yielding consisted in the propagation of plastic strain, as manifested by the Lüders' lines; such spreading being, doubtless, into regions previously affected by plastic strain but of so limited amount or partial distribution that the aggregate effect indicated by the extensometer was very small.†

As before described, the lines spread on both surfaces simultaneously, and the advancing front of a line of yielding extended across the width of the tube wall. The stress in the material immediately proximate to the advancing front must, for obvious reasons, be incalculable; but the result was to give Luders' lines, which, on the whole, had regular and definite inclinations. The fact that the lines usually appeared at an end of a specimen did not vitiate this regularity, except, in some cases close to that end. The applied loadings at the yield point were undoubtedly not those initiating elastic breakdown, but were those under which the plastic strain propagation became possible, or, at any rate, was actually effected.

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* Dr. J. Muir, "On the Overstraining of Iron by Tension and Compression." "Proc." Roy. Soc., A 77, 1896, p. 278.

Mr. J. J. Guest, "Ductile Materials under Combined Stress." "Proc." Phy. Soc., Vol. XVII., p. 208.

† Mr. G. T. Beilby's conclusions concerning the formation, between slipping cleavage surfaces, of mobile material which almost immediately hardens into a vitreous or amorphous phase has an important bearing on the whole question of the yield point, and very probably on the whole strain history from the crystalline elastic limit to the yield point.

See "Proc." Roy. Soc.; also "Engineering," May 19, 1911, p. 660.

The maximum stresses in the tubes (at the inner surface), calculated on the assumption that they were still perfectly elastic at the commencement of propagation of the large plastic yield strain, are the stresses given in the Tables A, B, C, D. They may be regarded as approximately the yield point stresses—*i.e.*, the stresses under which rapid extension of plastic yielding was precipitated.

The tabulated stresses show the same result of other experiments on combined stress—viz., that the maximum shear stress in the material is approximately a constant at the yield point. The main use of the calculated stresses has been to find from them the ratio: longitudinal stress/hoop stress, a ratio that was given a wide range in the experiments. Uncertainties in the stresses make a difference in the ratio that is small in comparison with the extent of its range.

The Applied Stresses and the Directions of the Lüders' Surfaces.

It will be seen from Tables A, B, C, D that there is, with a few exceptions, a regular variation of the inclination of the Lüders' lines corresponding to the ratio between the longitudinal stress and the hoop stress.

The total variation of angle in the author's tests was about 8 deg.—viz., from about 50 deg. (to the axis)—with end pressure only, to about 42 deg. with hoop tension four times the longitudinal compressive stress. With these stresses equal in intensity, the angle was 45 deg. or very nearly. The angles just quoted are the angles on the interior or exterior surfaces reduced to the corresponding angle at the mid-wall of the tube (*see* p. 318). Since it was possible that the alteration of angle might be due to the tubular form of the specimens and the particular circumstances of the testing, the last four tests of Table D were made. In the latter water pressure was applied to the exterior surface simultaneously with longitudinal tension—*i.e.*, the stresses were reversed from the directions in the other tests. The results show the same or a very similar relation between the angles of the lines and the directions of the stresses—*e.g.*, the lines were inclined at about 48 deg. to the direction of the hoop compression (unaccompanied by longitudinal tension), although they were now at 42 deg. to the axis of the tube. These results show that the Lüders' lines and surfaces do not follow lines of weakness due to, or are materially affected by, the process of manufacture; and they also indicate that the directions of the lines are not affected by instability (" secondary flexure ") of the tube walls under the applied forces. Confirmation of the latter is the similarity of results from tubes having different ratios of thickness to diameter (*see* Tables). Also these results are some evidence that the form of the specimens allows (comparatively) free development of the lines and surfaces of yielding. Had the tube walls been thicker (or the specimens solid and strained in torsion) it is possible that the whole thickness of wall would not have been brought to the yielding condition at the same time, and that the propagation of the strain would not have been so free; for even with the thicknesses experimented upon the angle of a Lüders' surface varied through the thickness.

Conclusions.

The yielding of mild steel tubes under longitudinal and hoop stresses of opposite sign consists in rapid spreading of plastic strain along lines of definite inclination to the axis of the tube. The mean inclination of the lines or surfaces of interior disturbance is the same as the mean of the surface Luders' lines (with the apparent exception of the lines at 90 deg. in tests in end pressure only).

With longitudinal stress p_1 and hoop stress p_2 (of opposite sign to p_1) the angle of inclination a and the ratio p_1/p_2 show variations which are correlated thus :—

a is 45 deg. when $p_1/p_2=1$.

a increases from 45 deg. to about 50 deg. while p_1/p_2 varies from 1 to ∞.

a decreases from 45 deg. to about 42 deg. while p_1/p_2 varies from 1 to 0·25.

The inclinations are the same to the direction of a simple stress whether the stress is a push or a pull; and given two stresses of opposite sign at right angles to each other, and of unequal intensity, the lines were more inclined to the direction of the larger stress, whether this was tensile or compressive.

These corresponding variations in a and in p_1/p_2 seem to be related as cause and effect.

Assuming this to be the case, it would appear that a shear stress of a certain maximum intensity will not be the only criterion for the condition of yielding. If a (half) difference of the principal stresses of given amount be the only condition for propagation of the yield plastic strains, the author fails to see why there should be any alteration of inclination of the Luders' surfaces so long as this given amount obtains, whatever the relative intensities of the principal stresses. Given the qualitative effect of change of inclination, the stress quantitative effect can hardly be negligible.

Application to other Tests of Mild Steel and of Wrought Iron.

Essential conditions for good development of the Lüders' lines appear to be that the dimensions of the specimen perpendicular to the direction of the greatest and least principal stresses should be small in comparison with the other two dimensions; and that the surface of the specimen should be in a state such that the traces of the internal disturbances may become visible.

The latter condition is often fulfilled by the scale on the specimen, as in the author's tubes (though occasionally a batch of tubes would show the lines so feebly as to necessitate their rejection). Surface polishing of the specimens also causes the lines to show clearly.

In specimens on which the lines either do not appear or only appear feebly or irregularly, it seems highly probable that the yielding—well defined and rapid as it is when care is taken to have uniform distribution of stress—will nevertheless be a process similar in kind to that of the yielding of the author's tubes.

In this connection the remarkable conclusion of Mr. L. Bairstow has an interesting bearing—viz., that "below the static yield point, iron and steel appear to be capable of maintaining an unstable condition against cyclical variations of stress which ultimately produce a considerable change of length. The first application of the maximum stress may show only a scarcely measurable extension, in spite of the fact that extension of thousands of times the amount may be obtained without any change in the cycle of stresses." *

On the other hand, for static (*i.e.*, gradually applied) stress, it would appear that increasing stress brings the specimen into a state of instability, which culminates in the rapid breakdown of the yield. Under this view, yield point stresses are those not appertaining to any elastic limit, but are those which precipitate rapid yielding, from an unstable condition, of material which has already experienced a history of elastic breakdown.

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* "Iron and Steel under Cyclical Variations of Stress." National Phys. Lab., "Collected Researches," Vol. VI., 1910, p. 181.

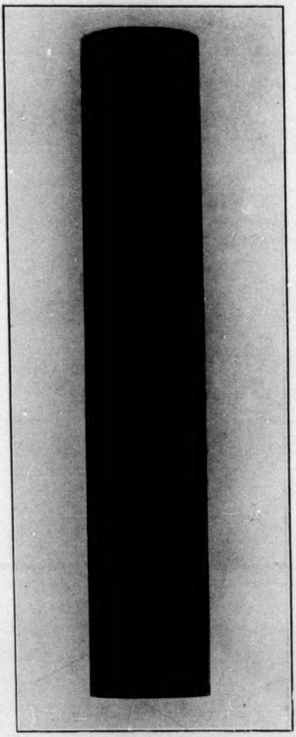
If it be assumed that the inclinations of the Lüders' surfaces of these experiments are those of unrestricted development (*see* p 319) of the process of the yielding, specimens exhibiting irregular lines of indefinite inclination would apparently have yielded under conditions which hampered the process, *e.g.*, the neighbourhood of material, in a thick tube or solid specimen, not yet stressed to a condition of instability. In such a case the yield point would probably be retarded, and the yield stresses somewhat higher.

The expense of apparatus and material for the foregoing experiments was defrayed partly by a grant from the University of Liverpool and partly from a grant from the "Bryan Donkin" fund made by the Institution of Mechanical Engineers. The work was done in the Walker Engineering Laboratory of the University of Liverpool; and the author desires to thank the Director of the Laboratory, Prof. W. H. Watkinson, for affording facilities therein for carrying out the tests. He also wishes to thank Mr. W. Donaldson, the Chief Instructor of the Laboratory, for many valuable suggestions, and he is grateful to those students who occasionally assisted him with the tests.

APPENDIX.

The Relation between the Inclinations of the Lüders' Lines and those of the Surfaces of Yielding.

The experiments described in the foregoing Paper show the Lüders' lines to be inclined to a generator at angles varying from about 50 deg. to about 42 deg., according to the stress system in force when the metal yielded. Assuming, for the argument of this appendix, that the lines are the traces



Specimen X. 12. Table C.
Inclination (to axis) of lines on exterior surface between 47¼°—48¾°.

FIG. 3.

To face p. 324.

TABLE A.—*Cold Drawn Mild Steel Tubes. Annealed, except where otherwise stated.*

Bore, thickness, &c.	Specimen.			Yield point, principal stresses.‡			Maximum shear stress.	$\frac{P_1}{P_2}$	Inclination to axis of tube of the Lüders' lines.								Remarks.
	No.	Test.*	Length.	Tons per square inch.					Surface on which the lines are measured.	Range of variation in inclination.	Number of lines measured.	Average angle of inclination of lines on exterior surf.	Average angle of inclination of lines on interior surf.	Average angle reduced to corresponding angle at the mid-wall of the tube.§ $=\alpha_0-0{\cdot}75°$ $=\alpha_i+0{\cdot}75°$			
				Long. P_1	Hoop P_2	Radial P_3											
				Compressive stress + *ve*.													
							Tons per sq. in.					α_0	α_i		$=\alpha$		
3 in. bore, 14 Gauge, about 0·08 in. thick.	II.B1	...	8·75″	4·50	−13·50	0·645	9·00	0·33	Interior	42·0°–43·0°	Several	...	42·2°		42·95°	43·6″	......
	II.22	1	12·0″	3·60	−10·80	0·52	7·20	0·33	Exterior	44·5°–45·5°	2	45·0°	...		44·25°		
	II.B4	2	8·75″	4·25	−13·50	0·645	8·87	0·32	Lines of former test broadened and extended								
	†II.9	...	7·25″	3·80	−7·80	0·375	5·80	0·5	Interior	42·6°–44·3°	4	...	43·4°		44·1°		......
	†II.3	1	15·0″	6·78	−6·74	0·32	6·76	1·0	Interior	...	1	...	45·0°		45·75°	45·5°	......
	†II.5*b*	...	7·0″	7·08	−7·42	0·355	7·25	0·96	Interior	All about 44°	Several	...	44·0°		44·75°		
	II.21	2	15·0″	7·80	−7·80	0·37	7·80	1·0	Exterior	45·0°–47·0°	4	46·3°	...		45·55°		
	†I.2*a*	2	18·0″	8·20	−8·10	0·385	8·15	1·0	Interior	Both at 45°	2	...	45·0°		45·75°		
	†I.5	1	7·0″	8·30	−8·05	0·38	8·17	1·03	Exterior Interior	46·0°–47·0° All about 44°	3 Several	46·5° ...	... 44·0°	45·75° 44·75°	45·25°		
	I.11	2	9·0″	9·25	−9·25	0·47	9·25	1·0	Exterior	46·0°–46·4°	2	46·2°	...		45·45°		
	VII.3	...	4·0″	12·7	−13·8	0·65	13·25	0·92	Exterior	46·5°–47·5°	Several	47·0°	...		46·25°		Not annealed
	VII.4	....	7·0″	12·9	−14·10	0·66	13·50	0·92	Exterior	All about 46°	Several	46·0°	...		45·25°		Not annealed
	†V.2	...	6·1″	11·25	−11·25	0·625	11·25	1·0	Exterior	46·5°–47·5°	Several	47·0°	...		46·25°		Not annealed
	†V.3*a*	...	19·0″	11·10	−11·10	0·61	11·10	1·0	Exterior	All about 46°	Several	46·0°	...		45·25°		Not annealed
	†II.8	1	7·25″	8·80	−4·40	0·217	6·6	2·0	Interior	45·0°–46·5°	Several	...	46·0°		46·75°		......
	VII.5	...	7·0″	16·63	−9·24	0·435	12·93	1·8	Angles not measurable			...	...		......		Not annealed
	II.24	...	6·1″	10·73	−3·81	0·182	7·27	2·8	Exterior Interior	50·0°–52·0° 47·0°–51·0°	3 3	51·0° ...	... 48·7°	50·25° 49·45°	49·0°	48·2°	......
	I.11	1	9·0″	13·42	−4·58	0·232	9·00	2·9	Exterior	Both at 48·25°	2	48·25°	...		47·5°		
	II.22	2	12·0″	10·9	−2·56	0·122	6·73	4·25	Exterior	47·4°–50·2°	3	48·9°	...		48·15°	49·0°	......
	II.B4	1	8·75″	14·4	−2·82	0·135	8·60	5·1	Exterior	48·7°–49·2°	3	49·0°	...		48·25°		
	II.21	1	15·0″	12·3	−2·56	0·122	7·43	4·80	Exterior	51·0°–52·0°	2	51·5°	...		50·75°		

* Where more than one test has been made on the same specimen, a number, denoting the order of the making of the test, is given in this column.

† The yield point stresses (but not the angles of the Lüders' lines) of specimens marked † were given in a Paper by the author, "Proc." Inst. Mech. Eng., 1909, No. 4.

‡ These are the stresses at the interior surface of the tube, calculated, on the assumption that the tube is perfectly elastic, from the loads at the yield point.

§ *See* p. 318.

TABLE B.—*Hot Drawn Mild Steel Tubes. Annealed.*

Bore, thickness, &c.	Specimen.			Yield point, principal stresses.‡			Maximum shear stress.	$\frac{P_1}{P_2}$	Inclination to axis of tube of the Lüders' lines.								Remarks.
	No.	Test.*	Length.	Tons per square inch.					Surface on which lines are measured.	Range of variation in inclination.	Number of lines measured.	Average of angles of inclination for exterior surface.	Average of angles of inclination for interior surface.	Average angle reduced to corresponding angle at mid-wall of tube.§ $=\alpha_0-1{\cdot}25°$ $=\alpha+1{\cdot}25°$			
				Long. P_1	Hoop P_2	Radial P.											
				Compressive stress + *ve*.													
												α_0	α_i		$=\alpha$		
pecimens cut from same length of Tubing. 2¼ in. Thickness about 0·128 in, (10 Gauge.)	†III.1	2	14·0″	8·43	−8·52	0·743	8·47	1·0	Exterior Interior	45·3°–46·5° Several lines about	5	45·9° ...	... 44·0°	44·65° 45·25	44·95°	44·85°	
	†III.2	2	9·0″	8·26	−8·30	0·725	8·28	1·0		Lines not measurable							
	†III.5	2	9·0″	8·12	−8·20	0·717	8·16	1·0	Exterior	...	1	45·7°	...		44·45		......
	†III.6	2	9·0″	8·28	−8·53	0·745	8·40	0·97	Exterior	45·5°–46·2°	2	45·9°	...		44·65°		
	†III.7	3	9·0″	8·55	−8·60	0·751	8·57	1·0	Exterior Interior	... ...	1 1	46·8 ...	... 43·8°	45·55 45·05°	45·30		
	†III.8	2	8·0″	8·05	−8·38	0·732	8·21	1·0	Lines of former test broadened								
	†VIII.2	2	4·5″	9·80	−9·70	0 847	9·75	1·0		Lines not measurable					......		Not annealed
	†III.3	1	4·5″	8·58	−8·60	0·751	8·59	1·0		Do. do.							
	III.10	2	4·1″	8·15	−8·15	0·713	8·15	1·0		Do. do.							
	†III.7	2	9·0″	10·41	−5·86	0·512	8·13	...		Do. do.							
	†III.8	1	8·0″	11·74	−4·81	0·420	8·27	2·44	Exterior	46·8°–48·6°	6	47·9°	...		46·65°	47·3	
	†III.9	...	8·0″	11·50	−3·82	0·335	7·66	3·0	Exterior Interior	46·8°–49·4° ...	6 ...	48·5° About	... 46·0°		47·25°		
	III.11	...	4·8″	11·86	−3·60	0·315	7·73	3·3	Exterior Interior	48·4°–50·0° 46·5°	4 2	49·2° ...	... 46·5°	47·95° 47·75°	47·85°		......
	III.10	...	4·1″	12·80	−3·20	0·280	8.00	4·0	Exterior Interior	... 46·2°	1 2	49·0° ...	... 46·2°	47·75° 47·45°	47·60		
	†III.1	1	14·0″	16·1	0	0	8·05	∞	Interior	About 50°	...	...	...		......		Also lines at 90°
	†III.2	1	9·0″	16·2	0	0	8·10	∞	Exterior	47·8°–52·0°	2	50·5°	...		49·25°		Do. do.
	†III.5	1	9·0″	14·78	0	0	7·39	∞	Exterior	49·0°–52·0°	3	50·5°	...		48·75°		Do. do. Not annealed

* Where more than one test has been made on the same specimen, a number denoting the order of making the test is given in this column.

† The yield point stresses (but not the angles of the Lüders' lines) of the specimens marked thus † were given in a Paper by the author, "Proc." Inst. Mech. Eng., 1909, No. 4.

‡ These are the stresses at the internal surface of the tube, calculated, on the assumption that the tube is perfectly elastic, from the loads at the yield point.

§ *See* p. 318.

TABLE C.—*Cold-Drawn Mild Steel Tubes. Annealed.*

Bore, wall-thickness &c.	Specimen.			Yield point, principal stresses.*			Maximum shear stress.	$\frac{P_1}{P_2}$	Inclination to axis of tube of Lüders' lines on exterior surface.					Remarks.
				Tons per square inch.										
	Number.	Test.	Length.	Long. P_1	Hoop P_2	Radial P_3			Number of lines measured.	Range of variation in inclination on specimen.	Average of angles of inclination. a_o	Average angle reduced to corresponding angle at mid-wall of tube $=a_o-1·25°$.†		
				Compressive stress + *ve.*										
Specimens cut from same length of tubing 2·5 inches bore. 11 Gauge (about 0·118 inch thick).	X.1	...	8·0″	3·4	−10·2	0·88	6·80	0·33	3	45·2°–46·6°	45·7°	44·45°		......
	X.7	...	8·1″	5·2	−10·4	0·87	7·80	0·5	3	45·5°–47·0°	46·3°	45·05°		......
	X.6	...	8·1″	6·6	−6·8	0·57	6·70	1·0	2	46·5°–48·0°	47·3°	46·05°	45·5°	......
	X.10	1	8·1″	8·56	−8·8	0·735	8·68	0·97	2	46·0°–46·2°	46·15°	44·9		......
	X.11	2	7·9″	7·96	−8·0	0·665	7·98	1·0	2	46·0°–47·2°	46·6	45·35		......
	X.13	2	9·0″	8·05	−8·0	0·665	8·02	1·0	3	47·0°–47·7°	47·4°	46·15°		......
	X.14	2	5·8″	8·15	−8·20	0·67	8·17	1·0	1	...	46·0°	44·75°	47·1°	......
	X.2	...	8·1″	9·4	−4·7	0·395	7·05	2·0	2	47·2°–48·8°	48·0°	46·75°		......
	X.9	...	8·0″	9·8	−4·9	0·42	7·35	2·0	1	...	48·7°	47·45°		......
	X.3	...	8·1″	12·6	−4·2	0·35	8·4	3·0	2	47·7°–48·2°	48·0°	46·75°	48·1°	
	X.5	...	8·0″	9·3	−3·1	0·26	6·2	3·0	4	49·5°–51·0°	50·3°	49·05°		......
	X.8	...	8·1″	9·6	−3·2	0·26	6·4	3·0	2	50·3°–51·6°	51·0°	49·75°		......
	X.12‡	...	15·0″	12·0	−4·0	0·335	8·0	3·0	3	47·2°–48·8°	48·1°	46·85°		
	X.10	2	8·0″	12·4	−3·2	0·26	7·8	4·0	Lines of first test broadened					
	X.11	1	7·9″	12·4	−3·1	0·26	7·75	4·0	2	48·0°–49·2°	48·6°	47·25°		
	X.13	1	9·0″	12·73	−2·30	0·191	7·52	5·5	3	49·5°–50·0°	49·7°	48·45°	48·0°	......
	X.14	1	5·8″	13·25	−2·32	0·193	7·8	5·7	2	48·7°	48·7°	47·45°		......

* These are the stresses at the internal surface of the tube calculated on the assumption that the tube is perfectly elastic, from the loads at the yield point.

† *See* p. 318. ‡ Illustrated in Plate Fig. 3.

TABLE D.—*Specimens Bored and Turned from Hot-Drawn Mild Steel Tubing.*

For detail of heat treatment after lathe-work, *see* p. 308.

	Specimen.			Load on testing machine.	Water pressure.	Longitudinal load due to water pressure.	Net longitudinal load on specimen.	Yield point, principal stresses, tons per sq. in. ‡			Maximum shear stress.	$\frac{P_1}{P_2}$	Inclination to axis of tube of Lüders' lines on exterior surface.					Remarks.
	No.	Length.	Mean thickness of tube wall.					Long. P_1	Hoop P_2	Radial P_3			Number of lines measured.	Range of inclination on specimen.	Average angle of inclination. a_0	Average angle reduced to corresponding angle at mid-wall of tube. $=a_0-1·0°$*		
								Compressive stress + *ve.*										
				Tons.	Lb. per sq. in. *Internal*	Tons.	Tons.				Tons per sq. in.					$=a$		
Cut from same length of Tubing. Bore 2¼ in. Thickness ⅛ in.	2	8·0″	0·099″	11·19	2,530	−7·29	3·90	4·23	−17·05	1·13	10·64	0·25	2	43·6°	43·6°	42·6°	41·8°	
	9	4·0″	0·0985″	12·56	2,870	−8·25	4·31	4·70	−19·62	1·28	12·16	0·24	4	41·5°–43·5°	42·0°	41·0°		......
	3	8·0″	0·098″	15·72	1,740	−5·00	10·72	11·75	−11·75	0·78	11·75	1·0	6	43·0°–46·8°	45·0°		44·0°	
	1	8·0	0·098″	17·51	435	−1·25	16·26	17·85	−2·97	0·195	10·41	6·0	5	47·2°–47·6°	47·4°		46·4°	......
	0	4·0″	0·0985″	14·10	270	−0·78	13·32	14·55	−1·96	0·121	8·26	7·4	1	...	46·5°	45·5°	45·3°	......
	8	4·0″	0·0985″	15·70	310	−0·84	14·81	16·15	−2·09	0·138	9·12	7·7	2	45·6°–46·6°	46·1°	45·1°		......
	4*a*	3·15″	0·0985″	15·26	0	0	15·26	16·60	0	0	8·30	∞	1	...	48·6°		47·6°	......
					External													
	4	4·75″	0·099″	...	2,820	...	0	0	19·00	0	9·50	0	2	43·0°–43·5°	43·3°		42·3°	......
	6	9·75″	0·089″	...	3,050	(−4·00)^1	(−4·00)^1	(−4·80)^1	(22·9)^2	0	(13·85)	(0·21)	7	41·3°–45·0°	43·3°	42·3°	41·7°	......
	7	9·8″	0·0885″	...	3,020	(−3·95)^1	(−3·95)^1	(−4·80)^1	(23·4)^2	0	(14·1)	(0·205)	18	39·3°–45·0°	42·2°	41·2°		Collapsed at 3,240 lb. per sq. in. external press
	5	9·75″	0·0895″	−13·6	1,220	(−1·59)^1	(−15·19)^1	(−18·3)^1	(9·12)^2	0	(13·7)	(2·0)	2	46·7°–47·0°	46·8°		45·8°	Central part of tube in the jacket
				−13·6	0	0	−13·6	−16·4	?^{3}	0	...	...	2	50·5°–52·5°	51·5°		50·5°	Ends projecting from the jacket

1. Loads and stresses doubtful on account of the uncertainty of action of the cup leathers in the jacket apparatus, Fig. 1.
2. Stresses doubtful on account of the influence of the ends of the specimens, which projected beyond the jacket.
3. Hoop stress of unknown (probably small) amount due to the influence of the water pressure over the central 4½ in. of the specimen.

‡ These are the stresses at the interior surface, calculated from the loads at the yield point on the assumption that the tube is perfectly elastic.

* *See* p. 318.

on the tube wall of surfaces of yielding, these surfaces are not those on which the shear stress is greatest, except (presumably) in the case where the lines are at 45 deg. to a generator. The surfaces of maximum shear stress in the material, of which the last-mentioned lines may be the traces, are, of course, planes containing a normal to the tube wall and being also at 45 deg. to a generator.

An explanation of the variation of angle (which presented itself during the course of the experiments) was that the material might perhaps be giving way on planes or surfaces of weakness in the tube in a manner like the following. A Lüders' line not at 45 deg. might be the trace of a plane B (whose inclination is nevertheless at 45 deg., or thereabouts, to a generator) which does not contain a normal to the cylindrical surface. The shear stress on B, though less than that on a plane A of maximum shear stress, might be sufficient to overcome the shearing resistance on B; and the stress on A, though greater than on B, might not be great enough to overcome the greater resistance to shear along A. Let Fig. 1 represent a plane section of the tube wall (supposed straight instead of curved for the sake of clearness), the section being normal to a generator. All such planes as B will cut this section in some line *cd*; let ϕ be the angle between *cd* and a normal *ab* to the tube wall.

Let each plane of the series B be fixed by its trace *cd* and its inclination to a generator. Let the principal stresses be p_1, p_2, p_3, where p_1 is parallel to a generator (the longitudinal stress), p_2 is perpendicular to a generator, and tangential to the tube wall (the hoop stress), p_3 is normal to the tube wall.

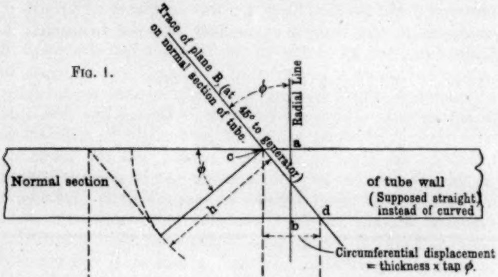
Then of all the planes whose traces *cd* make the same angle ϕ with *ab*, the one on which the shear stress is greatest is inclined to p_1 at an angle μ such that

$$\cos^2\mu=\frac{1}{2}\frac{(p_2-p_1)^2\cos^2\phi+(p_3-p_1)^2\sin^2\phi}{\{(p_2-p_1)\cos^2\phi+(p_3-p_1)\sin^2\phi\}^2}$$

The inclination μ of the plane of maximum shear for any value ϕ may thus be found for any system of the principal stresses, p_1, p_2, p_3. Thence the intensity of this maximum shear stress on the plane may be found in the usual way.

The traces of these planes on the two surfaces of a tube will be (as shown in Fig. 2) at a distance from each other, measured circumferentially along the mid-wall surface, nearly equal to thickness × tan ϕ. This distance has been called the "circumferential displacement" in the foregoing Paper. The above formula facilitates the finding of the plane of maximum shear stress given the displacement between a pair of its traces on the inner and outer surfaces of a tube of given thickness subjected to given values of p_1, p_2, and p_3. The angles μ of such planes, and the comparative values of the shear stress for certain values of ϕ are given in Table E for each of certain stress systems. Also the angles θ which the traces of these planes on the mid-tube wall make with a generator are given in the Table, θ being calculated from the relation,

FIG. 1.


Trace of plane B (at 45° to generator) on normal section of tube.
Radial Line
ϕ
a
c
ϕ
h
Normal section
of tube wall
(Supposed straight instead of curved)
d
b
Circumferential displacement = thickness × tan ϕ.

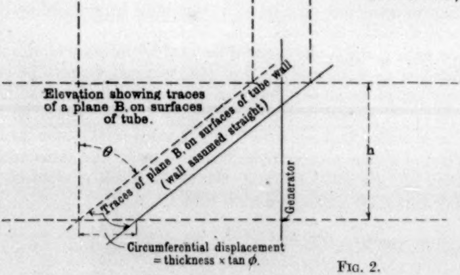

Elevation showing traces of a plane B on surfaces of tube.
Traces of plane B on surfaces of tube wall (wall assumed straight)
θ
h
Generator
Circumferential displacement = thickness × tan ϕ.

FIG. 2.

$$\tan\theta = \frac{\tan\mu}{\cos} \text{ (\textit{see} Figs. 1 and 2 above).}$$

Table E illustrates that the maximum shear on planes, which, while at or about 45 deg. to a generator, are skew to

TABLE E.

Stress system.	Plane of maximum shear stress passing through the line $\phi=0°$.			Shear stress on the plane $\phi=0$, $\mu=\theta$.	Plane of maximum shear stress passing through the line $\phi=15°$.			Shear stress on the plane $\phi=0$, $\mu=\theta$.	Plane of maximum shear stress passing through the line $\phi=30°$.			Shear stress on the plane $\phi=0$, $\mu=\theta$.
	Inclination to generator $=\mu$.	Inclination of trace to generator $=\theta$.	Shear stress on plane.		Inclination to generator $=\mu$.	Inclination of trace to generator $=\theta$.	Shear stress on plane.		Inclination to generator $=\mu$.	Inclination of trace to generator $=\theta$.	Shear stress on plane.	
$p_1=-p_2=11\cdot5p_3$	45°	45°	100	100	44° 24′	45° 24′	98·3	99·99	42° 51′	46° 58′	92·8	99·76
$p_1=-4p_2=40p_3$	45°	45°	100	100	44° 55′	45° 55′	98·55	99·95	44° 42½′	48° 49′	96·4	99·11
$p_1=-8p_2=80p_3$	45°	45°	100	100	44° 58′	45° 57′	99·15	99·94	44° 55′	49° 1½′	97·45	99·01
$p_2=0$, $p_3=0$	45°	45°	100	100	45° 0′	46° 0′	100	99·94	45° 0′	49° 6′	100	98·97

the surface of the tube, differs very little from the maximum shear stress in the material. The traces of these planes, for the stress systems of the Table (E), have inclinations very similar to those of the observed Lüders' lines for the same stress systems in the experiments. If, however, those planes were the actual planes of sliding, it would be very curious that the tube structure should be such as to give the regular and progressive values of α found.* (*See* Tables A, B, C, D in the Paper). But if the Lüders' lines were the traces of such planes, there would be the cicumferential displacement thickness$\times\tan\phi$ between a "pair" (*see* page 316 of the Paper) of the traces. This displacement would be greatest in the thicker tubes when stressed, so that one principal stress is much greater than the other. In the 10 G tube specimen, No. III. 7 of Table B, α is (about) 50 deg. for several lines; if the slipping occurred on that plane of maximum shear (at 45 deg. to a generator) which gives 50 deg. as the slope of the trace on the mid-wall surface, ϕ would be 33 deg., and the displacement would be $0{\cdot}128\times\tan 33^\circ=0{\cdot}08$ in. If we suppose 40 deg. to be the slope of the planes of slipping (or about the angle commonly supposed to be the angle of slipping for compression alone) then, to give a trace at 50 deg., ϕ would be 45° 50′, and the displacement 0·13 in. Now, displacements of such magnitudes are much too large to be mistaken; indeed, there appeared to be no displacement except in one pair of lines on the above tube, and that was about 0·04 in. But it was very difficult to measure displacement (if any) with accuracy, the limit of accuracy being about 0·03 in. for this specimen. If we take a displacement of 0·03 in., ϕ will be

$$\tan^{-1}\frac{0{\cdot}03}{0{\cdot}128}=13^\circ\ 10',$$

and to give $\alpha=50^\circ$, the inclination of the plane of slipping will be 49° 15′. It is clear, then, that the slope of the planes of slipping and the inclination of their traces on the mid-wall surface must be practically the same for the lines at 50 deg.† on this specimen. If m is the inclination of a plane of slipping whose mid-wall trace is inclined α, then,

$$\tan(\alpha-m)=\frac{\tan\alpha(1-\cos\phi)}{1+\cos\phi\tan^2\alpha}.$$

---

* α is the angle of the Lüders' lines to the tube axis reduced to the corresponding angle at the mid-wall surface (*see* page 318 of the Paper).

† There were also other lines, due to other stress systems, on this tube.

Taking ϕ, as above, equal to 13° 10′, $(\alpha - m)$ has the approximate value 0·75° when α has any value between 40 deg. and 50 deg.; hence, in the absence of displacement more than 0·03 in., all the planes of slipping in the 10 G tubes must be at practically the inclination of the corresponding Lüders' lines.

The same conclusion was reached from examination of all the eight tubes of different thicknesses on which displacement was looked for. Taking 0·03 in. as the error of measurement of displacement in the thinnest tubes tested (14 G), then $\phi = 20{\cdot}5°$, and $\alpha - m = 1{\cdot}8°$ (about) when α does not differ more than ± 5 deg. from 45 deg. Thus, even in these thinnest tubes, the observed variation of α could not be explained by skewness of the planes of slipping.

ABSTRACT.

Previous investigations have shown that Lüders' lines on specimens of mild steel and wrought iron, strained in tension, are inclined at about 50 deg. to the axis of pull. For tests in compression the information available is not precise, and though the angle of the lines with the direction of the compression is commonly understood to be about 40 deg., some doubt has been thrown on this point.

The author had found previously that the lines are well developed on the surface of mild steel tubes. Since it was easy to obtain a compressive stress of practically uniform distribution in tubes under end pressure, while at the same time a hoop tensile stress could be induced by internal fluid pressure, the author confined his attention to the lines on tubular specimens. These were of mild steel, either hot or cold drawn, and most of them were annealed. They varied in bore from $2\frac{1}{2}$ in. to 3 in., and in thickness of wall from about 0·08 in. to 0·125 in. Four sets of tubes were tested under end loading, simultaneously (for the most part) with internal water pressure, these loadings being so arranged as to give at the yield point ratios of the longitudinal compressive stress to hoop tensile stress ranging from 0·25 to ∞.

The Lüders' lines on the outer surface appeared at the yield point indicated by the extensometer—*i.e.*, their appearance coincided with the commencement of the large "yield" strain. In all cases where there were lines on the inner and outer surfaces of a tube, an inner and outer line, and also the ends of these lines, were found to be radially opposite; showing that the lines, were traces of surfaces or canals of disturbance which passed through the tube wall, and indicating, moreover, that the disturbance spread spirally onwards, and not outwardly from a line initially formed on the more severely stressed inner surface.

The inclination α of the lines to the axis of the tube was found to vary in the following way for tubes under end load and internal pressure :—

$$\text{For } \frac{\text{longitudinal stress}}{\text{hoop stress}} = 0{\cdot}25, \ \alpha = 42 \text{ deg. (about).}$$

„ „ „ $= 1$, $\alpha = 45$ deg. (about).

„ longitudinal stress only, $\alpha = 50$ deg. (about).

For values between 0·25 and 1, and between 1 and ∞, a became progressively larger.

Some tests of tubes under external water pressure and longitudinal tension gave results similar to those above mentioned. The directions of the lines with respect to the axes of like stress were practically the same, though the inclinations to the axes of the tubes were now complementary to the values of a quoted above.

The conclusion is drawn that the Lüders' surfaces have the same or approximately the same inclination to an axis of simple pull or simple push. With stresses of opposite sign at right angles to each other the lines and surfaces are more inclined to the stress of greater intensity, and with equal intensities the surfaces are at about 45 deg. The author suggests that if a shear stress of given intensity be the only condition for that spreading of plastic strain which commences at the yield point, then there is no reason why there should be any variation in the angle of the Lüders' lines and surfaces. The variation suggests that not only the maximum shear stress—*i.e.*, the half difference of the greatest and least principal stresses—but also the magnitudes of the principal stresses are factors in producing the yielding condition in mild steel.

DISCUSSION.

Dr. Chree said Lüders' lines seemed to possess a double interest. If their first existence coincided in time with the ending of linearity in the stress-strain relation, their presence intimated that the ordinary mathematical equations had ceased to apply. In this connection their utility would be greater if the stress function supposed to determine the elastic limit were greatest at a point or small area visible to the eye, instead of, as in the present case, over the whole inner surface of a cylinder. The phenomenon seemed, however, also interesting in itself. The nature of the change in the material and the mode of propagation of the altered volume would be more easily studied if the altered material could be recognised in sections of the cylinders.

Prof. E. G. **Coker** asked whether the lines were a surface phenomenon produced by the stresses in the oxide.

Prof. C. A. M. **Smith** said that he differed from the opinion of Mr. Mason as to what actually took place at elastic breakdown. He thought there could be no doubt that the record of the appearance of the Lüders' lines on the tubes and the measurement of the angles of the spirals added considerably to our knowledge of what happened at the yield point. He thought the great point of the work was the relation of the spiral on the inner surface of the tube with that on the outer surface. While he thought that for the phenomena of elastic breakdown tubes were unsuitable, yet the specimens used for this investigation on Lüders' lines possessed the great advantage that the Lüders' lines could be traced both inside and outside the tube.

Mr. E. H. **Rayner** pointed out that the elastic constants in the two directions would probably be different, and that the compression might put on a screw shear resulting in lines only appearing in one direction.

Dr. A. **Russell** asked the author if his results threw any light upon the phenomena obtained when wires are drawn.

Mr. **Maclachlan** asked the author if he had ever tried polishing the surface of his tubes. His own experiments made on the compression of solids showed that the lines were not always spirals, but approximated to a section of the cylinder made by a plane inclined at 45 deg.

The following communications respecting this Paper have been received :—

Mr. G. H. **Gulliver** writes : Mr. Mason gives results which are in general agreement with those of Hartmann and of my own. The most important new observation is that with two unequal principal stresses, of opposite sign, the lines are more inclined to the direction of the larger stress, whether this is tensile or compressive, and that when the stresses are equal and unlike the inclination is 45 deg. to each. The latter observation is not in agreement with results obtained by the writer with twisted cylindrical bars, upon which the lines were found to be inclined at 50 deg. to the direction of principal tension, and the former differs from results obtained with bent prismatic bars; but these apparent contradictions only indicate the need for further experiments. Another interesting observation of Mr. Mason is that on reloading a tube which has been strained already beyond the yield point, the new lines always differ in inclination from the old ones. Is the inclination increased or decreased, and is the change always in the same direction? The general form of the surface of sliding—which in detail has irregularities on a scale corresponding with that of the structure of the metal—is easily determined in the more simple cases; but if the metal be supposed structureless, a helical line should not be the trace of a surface, but the extreme member of a family of helices of constant inclination. In the case of a thin tube, as Mr. Mason shows, the family is replaced by a helical surface of constant pitch, but a thick-walled tube might be expected to give a different result. With regard to the stated non-appearance of Lüders' lines on ordinary test bars, this is much more a question of the condition of the surface of the metal than of the relative dimensions of the bar. The lines will not be seen on a rough surface or on an ordinary turned surface, but they are quite noticeable on most mild steel bars and plates if the scale has not been removed before testing. The most accurate observations are made upon highly polished surfaces, but satisfactory photographic reproductions of such surfaces are difficult to obtain. Mr. Mason does not mention the occurrence of the continuous propagation of deformation in his specimens, as contrasted with the discontinuous propagation visible as Lüders' lines. It has been stated by Fremont that when care is taken to ensure uniform loading the propagation is continuous, and that, conversely, a discontinuous propagation indicates irregularity of loading. The writer disagrees with this, and would be glad to know of any observation of Mr. Mason bearing on the point. Referring to the case of lead, it may be noted that Humfrey observed markings, on specimens *etched after straining*, which, as shown in his photographs, have an inclination of approximately 50 deg. to the apparent direction of tension.

Mr. F. ROGERS writes: Having made an exhaustive investigation of Lüders' lines from certain aspects, the following conclusions may help to throw light on the nature of these strain effects in steel and soft iron. The following is a quotation from my Paper "Microscopic Study of Strain in Metals"* (p. 16): "Several years ago Prof. Ewing and the author were engaged in studying the nature of Lüders' lines, which, as is well known, appear obliquely to the direction of stress upon a suitably prepared surface of certain metals when stressed beyond the elastic limit. With further loading a line becomes a band of increasing width, within whose limits the surface of the specimen is visibly ruffled. With moderate magnification the wave-length of these undulations in soft irons—Swedish and Low Moor—was found to be from three to five times the average breadth of crystal grains in the specimen. The author has since found a similar relation between the wave-length—"of ruffling"—and size of grains in both normal and overheated samples of steels of the series referred to above, upon loading them statically in tension. Further, it is found that the ruffling of the surface, observed in fatigued samples of the normal class, has also a similar relation to the structure. The crests follow very irregular lines on the observed surfaces; this, and the variability of the wave-length, point to the dependence of the form, dimensions, and indeed existence, of the undulations upon the microscopical heterogeneity of the material." These investigations were made upon tensile test strips, say 4 in. tested length and ½ in. by ¼ in. section, with the whole of one face, 4 in. by ½ in., perfectly prepared beforehand for microscopic examination, and lightly etched. At the moment when the extensometer (Ewing's pattern) indicated in the least degree that the elastic limit had been exceeded the load was removed, and a Lüders' line was always found. Within this " line "—really a band, of course—slip bands could always, though with great labour, be found, and so also could the " ruffling " mentioned above. Incidentally, it is interesting that the Lüders' lines on surfaces thus prepared are perfectly permanent—that is to say, they will not alter or disappear so long as the specimen is prevented from rusting. Some of my samples look almost perfect now although they are about nine years old. There seems to be no important divergence of opinion between Mr. Mason and myself in those cases in which we have approached the problem apparently from the same points of view.

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* "Journal" Royal Microscopical Society, 1907, pp. 14-18.

The AUTHOR, in reply to Dr. Chree, said the experiments do not, in his opinion, show that the state of the material at the appearance of the Lüders' lines is one of perfect elasticity. Only the appearance of lines on the outside surface of the tubes was observed; possibly if the starting of scale disturbance on the inner surface had been observed, the coincidence between the bend on the stress strain curve and such initial disturbance might have been still more striking. But it is very difficult to believe, considering the constitution of the material, that the yield point and the Lüders' lines are not the symptoms of a collapse from a state in which some grains would have had large cleavage slips but for the relief of stress afforded by the surrounding material. If this view is correct, the equations of stress depending on the assumption of perfect elasticity may hold with approximate accuracy for small volumes, though they would not be correct for a " point in the material." With regard to the suggestion that it would be interesting if the Lüders' disturbance could be made visible in and through the wall thickness, it would seem to be hopeless that any indication would be visible under the microscope on a cut section of a tested specimen; while an end surface is liable to the irregularities of stress applied to it. The best evidence of the through-wall character of the disturbance is the presence, on some of the Table D specimens, not merely of pairs of opposite bands, but also of opposite pairs of fine lines which occur in the centres of the bands. The lines were the nuclei from which the bands spread out laterally.

Replying to Prof. E. G. Coker, the author believed the coating of scale to give evidence of plastic strain in the metal, because of the correspondence of the indications of the extensometer and the scale. If the scale indicated merely its own elastic or rupture limit, and not the disturbance in the steel, it is difficult to account for the pairs of opposite lines.

In reply to Prof. C. A. M. Smith, the author could only endorse the remark concerning the practical difficulty of deciding upon the relation between the " elastic limit " and the yield point.

In reply to Mr. E. H. Rayner, the Author relies upon the latter half of the experiments of Table D, in which the axes of tension and compression were interchanged in position in the tubes, to show that differences in the material, such as those mentioned by the speaker, had no sensible effect on the directions of the lines. With regard to the " screw " effect, it is possible that some instability in the testing machine might give rise to a very small amount of torque. But the effect of torque would be to turn the axes of stress round about a radius of the cylinder, and therefore the inclination of one set of spiral lines would be increased, while those of opposite hand on the same specimen would be diminished. Both sets of spirals on a specimen varied in the same way with change of stress system.

In reply to Mr. Maclachlan, the author did not polish any of his specimens. For the reasons given, he regarded the scale as a very delicate indicator. He intended, however, to test a few polished tube specimens.

With regard to the elliptical, and not spiral, lines on solid specimens, the Author would be extremely glad to have further details of the specimens and the testing. An elliptical line on a solid specimen meant a large area for a continuous surface of yielding.

In reply to Mr. G. H. Gulliver's remarks, the author writes that the angle of the lines on twisted cylindrical bars and on bent prismatic bars do not correspond with those on the tubes. The author agrees that further experiment is needed to explain this difference, as well, also, to elucidate the relation between the dimensions and form of the specimen and the angles of the surfaces of yielding. The Author does not intend it to be understood that, on re-loading a tube which had been strained to yield, the newly-formed lines had a different inclination when the stress system was similar to that of the first loading.* Only when the value of $\frac{p_1}{p_2}$ was altered from that in the first test was any change of angle observed. The propagation of strain in lines or bands does not appear to indicate irregularity in distribution of loading. Long lines—sometimes making a whole spiral revolution, or even more, of a tube—had inclinations sensibly constant over their lengths. Considerable variation of inclination would be expected if such irregular distribution were the cause of the strain propagation described in the Paper.

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* *See* p. 316.

XXXI. *A New Method of Approximate Harmonic Analysis by Selected Ordinates.* *By* SILVANUS P. THOMPSON, *F.R.S.*

RECEIVED MAY 11, 1911. READ JUNE 9, 1911.

1. The approximate harmonic analysis of periodic curves may be attained by a simple and accurate process, considerably shorter than any hitherto described. According to Fourier's theorem any periodic single-valued function of x may be expressed in terms of the series—

$$y = A_1 \sin x + A_2 \sin 2x + \ldots\ldots + B_1 \cos x + B_2 \cos 2x + \ldots\ldots$$

The process of harmonic analysis consists in finding the coefficients A_1, $A_2, \ldots\ldots B_1$, $B_2 \ldots\ldots$ of the several terms of this harmonic series.

In the ordinary processes, derived from Fourier's integrals, a number of equidistant ordinates are measured on the curve. Each ordinate is then multiplied by the sine of the angle at which it stands (or by the sine of that angle multiplied by the numeric of the harmonic to be found), the average of these products over a whole period being half the amplitude of the harmonic sought. This process yields the sine component of the harmonic in question. By substitution of cosine for sine in the multiplication the amplitude of the cosine constituent may be found. A method systematised by grouping prior to multiplication by the sines (or cosines) was propounded in 1903 by Runge*; and a shortened process based on this plan was brought by the author before the Physical Society† in 1904.

In the new method now to be explained no multiplication by sines or cosines is required. The sole process is the simple arithmetical averaging of selected ordinates, followed by certain further additions or subtractions.

The basis of the method lies in the easily-verified fact that if a series of $2n$ ordinates is measured at intervals apart of π/n, where n is the numeric representing the order of the harmonic, and if their values, *taken alternately positively and negatively*, are averaged over a whole period, the mean so obtained is either simply the amplitude of that harmonic or else is the sum of the amplitudes of that harmonic and of those of certain higher harmonics—namely, those the ordinal numeric of which is an odd multiple of n—the elimination of which is explained lower down. For cosine components the series of $2n$ ordinates must begin (or end) at the beginning (or end) of the period. For sine components the series must begin at $\pi/2n$ from the beginning of the period. Thus, to ascertain the value of the amplitude of the fifth cosine harmonic, all that is necessary is to average the 10 ordinates erected at 0°, 36°, 72°, &c., taken alternately positively and negatively; or in symbols,

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* "Zeitschrift für Mathematik and Physik," XLVIII., p. 443.

† "Proc." Physical Society, XIX, p. 443; read December 9, 1904.

$$B_5 = \tfrac{1}{10}(y_0 - y_{36} + y_{72} - y_{108} + y_{144} - y_{180} + y_{216} - y_{252} + y_{288} - y_{324});$$

and, similarly, for the fifth sine component,

$$A_5 = \tfrac{1}{10}(y_{18} - y_{54} + y_{90} - y_{126} + y_{162} - y_{198} + y_{234} - y_{270} + y_{\;\,0} - y_{\;\,2}).$$

This is on the assumption that there are no components of the

FIG. 1.

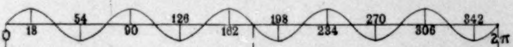


FIG. 2.

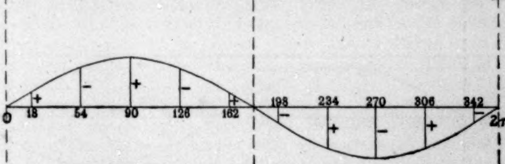
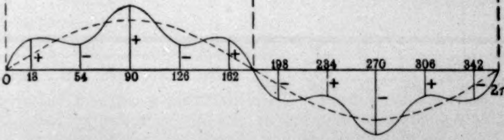


FIG. 3.



15th, 25th, 35th, &c., orders present. If such are present they must first be evaluated; for the true B_5 is the mean found above, minus $(B_{15} + B_{25} + B_{35} +$ &c.). The similar correction for A_5 is to deduct from the mean, as found above, the sum of $(-A_{15} + A_{25} - A_{35} +$ &c). If these higher harmonics are absent or if they are thus taken into account, the values obtained by the simple averaging will be true, and will contain absolutely no constituent due to any other harmonic, odd or even, of either higher or lower order.

2. The rationale of the process will be evident from a consideration of the figures which follow. If in Fig. 1 the base-

line 0 to 2π represent one period, then the curve represents a fifth sine harmonic; and mere inspection shows that its amplitude will be $\frac{1}{10}$ of the sum of the 10 equidistant ordinates *if taken alternately positively and negatively* as to their values as they stand. Fig. 2 shows a fundamental sine curve of the same period, with ordinates raised at the same points on the base-line; while Fig. 3 exhibits the curve compounded out of Figs. 1 and 2. The heights of the 10 successive ordinates of Fig. 3 are, in millimetres,

$$6, \quad 5, \quad 13, \quad 5, \quad 6, \; -6, \; -5, \; -13, \; -5, \; -6.$$

If we reverse the signs of the alternate ordinates we have

$$6, -5, +13, -5, +6, +6, -5, +13, -5, +6,$$

and the sum of these is 30, and the mean value is 3. Hence we learn that $A_5=3$. The ordinate at 90° is 13, and is the

FIG. 4

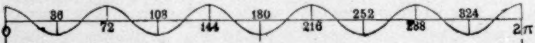


FIG. 5.

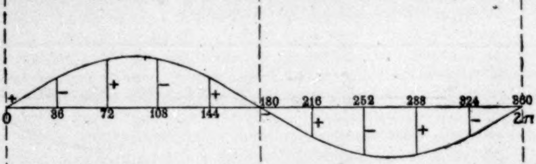
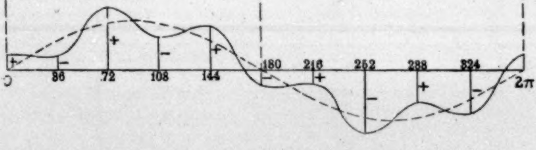


FIG. 6.



sum of A_5 and A_1; hence we learn that $A_1=10$, and the equation to the curve of Fig. 3 is $y=10 \sin x+3 \sin 5x$.

Similarly, Fig. 4 depicts a fifth cosine harmonic, and Fig. 6 gives this compounded with the fundamental sine curve. To ascertain the cosine component of the fifth order we must measure 10 ordinates, at every 36°, beginning at zero. These ten measured ordinates are

$$3, \quad 2{\cdot}9, \quad 12{\cdot}5, \quad 6{\cdot}5, \quad 8{\cdot}9, -3, -2{\cdot}9, -12{\cdot}5, -6{\cdot}5, -8{\cdot}9.$$

Changing the signs of alternate terms we obtain

$$3, -2{\cdot}9, +12{\cdot}5, -6{\cdot}5, +8{\cdot}9, +3, -2{\cdot}9, +12{\cdot}5, -6{\cdot}5, +8{\cdot}9.$$

The mean of these is 3. Hence the equation to the curve is

$$y=10 \sin x+3 \cos 5x.$$

It will be remarked that, in these two illustrations of the method, it would have sufficed to average over half a period. But had we taken an example containing any harmonic of an even order it would not have been so. For generality, it is necessary to average over a whole period.

Had we wished to ascertain the third harmonic constituents we should have had to use six equidistant ordinates at 60° apart. For the nth harmonic $2n$ ordinates must be taken π/n apart.

. In thus averaging from so few ordinates the question arises: do we really eliminate constituents of other orders? In the case of cosine constituents it is obvious from the mere symmetry of Fig. 5 that the equidistant ordinates of the fundamental, if taken alternately positively and negatively, add up to zero. The same is true of Fig. 2 if taken over the whole period, though not obvious (yet true) for each half period. For harmonics of other orders it is also true, except for the cases already mentioned of harmonics of a frequency which is an odd multiple of the frequency of the harmonic which is sought.

3. To prove that this same process of adding, with alternate signs, the ordinates spaced out at intervals of half a period of the harmonic sought, will, when averaged, give the amplitude of that harmonic, consider the sum S of the series of $2n$ terms

$$S=B_n \cos 0 - B_n \cos \pi + B_n \cos 2\pi - B_n \cos 3\pi \ldots\ldots$$
$$\ldots\ldots - B_n \cos (2n-1)\pi;$$

the value of which is obviously $2nB_n$. Hence B_n is the value obtained by summing up the $2n$ ordinates reckoned alternately positively and negatively, and dividing by $2n$.

4. The generality of the proposition that the fundamental is eliminated from the sum is easily demonstrated. For generality let us assume that the first ordinate is taken at an arbitrary angle, a, and that the succeeding ordinates are spaced apart at successive distances β, where β is equal to π/n, for the purpose of finding the amplitude of the nth harmonic. When we are finding sine constituents $a=\frac{1}{2}\beta=\pi/2n$. When we are finding cosine constituents we must take $a=0$. Then the sum required (for sines) is the following, to $2n$ terms:—

$$S=\sin a-\sin(a+\beta) +\sin(a+2\beta) -\sin(a+3\beta)$$
$$+\ldots\ldots-\sin\{a+(2n-1)\beta\}.$$

This is equivalent to

$$S=\sin a+\sin\{a+(\beta+\pi)\}+\sin\{a+2(\beta+\pi)\}$$
$$+\sin\{a+3(\beta+\pi)\}+\ldots\ldots+\sin\{a+(2n-1)(\beta+\pi)\}.$$

Then, by a known theorem, the sum of this series is

$$S=\frac{\sin 2n\dfrac{\beta+\pi}{2}}{\sin\dfrac{\beta+\pi}{2}}\sin\left\{a+(2n-1)\frac{\beta+\pi}{2}\right\}.$$

Inserting the values of a and β, we obtain

$$S=\frac{\sin(n+1)\pi}{\sin\dfrac{n+1}{n}\pi}\cdot\sin\frac{\pi}{2n}\{1+(2n-1)(n+1)\}.$$

This expression is of zero value for all integral values of n above unity, since $\sin(n+1)\pi$ vanishes, whether n be odd or even.

The corresponding series of cosines, with $a=0$, has for its sum

$$S=\frac{\sin(n+1)\pi}{\sin\dfrac{n+1}{n}\pi}\cdot\cos\frac{\pi}{2n}(2n-1)(n+1),$$

which likewise vanishes for all integral values of n above unity.

5. In dealing with those higher harmonics, the order of which is an odd multiple of the order that is being sought, we find that the sum does not vanish. For consider the ordinate y_1 at the position $\theta=\frac{1}{2}\frac{\pi}{n}$, which is the first position for finding the sine constituent of the nth order.

$$y_1=A_1\sin\theta+A_2\sin 2\theta+A_3\sin 3\theta+, \&c.$$

$$=A_1\sin\frac{1}{2}\frac{\pi}{n}+A_2\sin\frac{2}{2}\frac{\pi}{n}+A\ \sin\frac{3}{2}\frac{\pi}{n}+, \&c.$$

$$=\Sigma A_m\sin\frac{m}{2}\frac{\pi}{n};$$ where m has the successive values 1, 2, 3, &c.

$$=\Sigma A\ \sin\frac{m}{n}\frac{\pi}{2}.$$

If in any term of this series m/n is an even integer, then that term will vanish. If m/n is an odd integer, that term will have the value $\pm A_m$, the sign being + if m/n is 1, 5, 9, &c., and − if m/n is 3, 7, 11, &c. If m/n is not an integer, that one term will not vanish *per se*. But if we regard not simply y_1 but the whole series of ordinates, y_1, y_2, y_3, &c., spaced out at successive intervals of 2θ, over the whole period, then the sum, taken with alternate positive and negative signs, will be cumulative for odd integral values of m/n, and will have the value of nA_m, and will vanish for all values of m/n that are not integral. It is for this reason that the process of averaging for any harmonic requires either that the higher harmonics, the ordinal numbers of which are odd multiples of its ordinal number, be absent, or that they shall be separated evaluated, and so eliminated. Hence, in the schedules which are arranged below for carrying out the process, the higher harmonics are evaluated before the lower ones, and the fundamental last. A caution is necessary that the method may fail to give correct results if higher harmonics are present and are not thus eliminated.*

6. For curves of alternating electric currents and alternating voltages in which odd harmonics only are present, it suffices to average the selected ordinates over a half-period only. But for curves of valve-motions and other periodic quantities which contain even harmonics, the ordinates must be averaged over the whole period.

This process has been found to be specially well adapted to particular cases. For example, in dealing with the voltage curve of an alternator having in its armature six slots per pole, where it is known that probably the only harmonics present are the 5th, 7th, 11th and 13th, these can be evaluated directly without having to work out any others of the series.

Three different schedules for facilitating the process are given below, the first of them being a simple form suitable for valve-motion curves. The second is for odd harmonics only up to the 9th order. The third for odd and even harmonics up to the 7th. An example of an analysis by the first schedule is appended.

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* For cognate processes see Strachey, "Proc. Roy. Soc." XLII., p. 67 1887; Darwin, "Brit. Assoc. Report," 1883, p. 49; Burkhardt, "Jahresbericht der deutschen Mathematiker, Vereinigung," 1901-2-3, X., pp. 279-288, &c. Fischer-Hinnen, "Elektrotechnik und Maschinenbau," XXVII., 1909, p. 335.

SCHEDULE I.

(Simple schedule for valve-gear diagrams, &c.)

HARMONIC ANALYSIS BY SIMPLE AVERAGING OF SELECTED ORDINATES.

(Harmonics above those of the 3rd order assumed to be absent.)

To find A_1, A_2, A_3, B_1, B_2, B_3.

Divide the base-line of one period to read off in degrees, and measure off the ordinates as required:—

$$A_3=\tfrac{1}{6}(y_{30^\circ}-y_{90^\circ}+y_{150^\circ}-y_{210^\circ}+y_{270^\circ}-y_{330^\circ});$$
$$B_3=\tfrac{1}{6}(y_{0^\circ}-y_{60^\circ}+y_{120^\circ}-y_{180^\circ}+y_{240^\circ}-y_{300^\circ});$$
$$A_2=\tfrac{1}{4}(y_{45^\circ}-y_{135^\circ}+y_{225^\circ}-y_{315^\circ});$$
$$B_2=\tfrac{1}{4}(y_{0^\circ}-y_{90^\circ}+y_{180^\circ}-y_{270^\circ});$$
$$A_1=\tfrac{1}{2}(y_{90^\circ}-y_{270^\circ})+A_3;$$
$$B_1=\tfrac{1}{2}(y_{0^\circ}-y_{180^\circ})-B_3.$$

The mean ordinate A_0 can be found thus—

$$A_0=y_0-B_1-B_2-B_3.$$

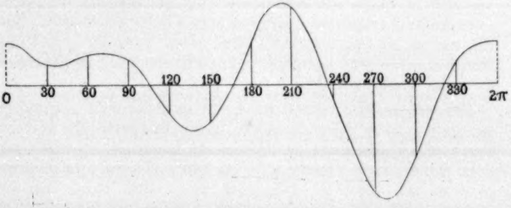

FIG. 7.

Example of the use of Schedule I. Analysis of the curve given in Fig. 7.

This curve was calculated from the equation $y=6\sin\theta+8\sin 2\theta+10\cos 2\theta-10\sin 3\theta$, and plotted to a scale of $\frac{1}{10}$th of an inch as unit. Another person then took off the ordinates from the curve and found them as follows:—

$y_0=10{\cdot}0$ $y_{90}=6{\cdot}0$ $y_{180}=10{\cdot}0$ $y_{270}=-25{\cdot}9$
$y_{30}=5{\cdot}0$ $y_{120}=-6{\cdot}8$ $y_{210}=18{\cdot}5$ $y_{300}=-17{\cdot}3$
$y_{45}=5{\cdot}3$ $y_{135}=-10{\cdot}9$ $y_{225}=10{\cdot}7$ $y_{315}=-4{\cdot}7$
$y_{60}=7{\cdot}2$ $y_{150}=-8{\cdot}9$ $y_{240}=-3{\cdot}4$ $y_{330}=5{\cdot}1$

Inserting these values in the schedule gave the following results:—

$$
\begin{aligned}
A_3 &= \tfrac{1}{6}(5{\cdot}0-6{\cdot}0-8{\cdot}9-18{\cdot}5-25{\cdot}9-5{\cdot}1) &&= -9{\cdot}90;\\
B_3 &= \tfrac{1}{6}(10{\cdot}0-7{\cdot}2-6{\cdot}8-10{\cdot}0-3{\cdot}4+17{\cdot}3) &&= -0{\cdot}013;\\
A_2 &= \tfrac{1}{4}(5{\cdot}3+10{\cdot}8+10{\cdot}7+4{\cdot}7) &&= 7{\cdot}87;\\
B_2 &= \tfrac{1}{4}(10{\cdot}0-6{\cdot}0+10{\cdot}0+25{\cdot}9) &&= 9{\cdot}98;\\
A_1 &= \tfrac{1}{2}(6{\cdot}0+25{\cdot}9)-9{\cdot}90 &&= 6{\cdot}05;\\
B_1 &= \tfrac{1}{2}(10{\cdot}0-10{\cdot}0)+0{\cdot}013 &&= 0{\cdot}013.
\end{aligned}
$$

The slight discrepancies, when compared with the original amplitudes used to calculate the curve, arise from the unavoidable errors in plotting or in reading the values of the ordinates of the curve as plotted. They do not arise from any want of accuracy in the method itself.

SCHEDULE II.

HARMONIC ANALYSIS BY AVERAGING OF SELECTED ORDINATES.

Odd Harmonics only up to the 9th order.

First find the correct base-line, so that the mean ordinate is zero. Then divide the base-line of one period to read in degrees, and measure off ordinates as required in the following formulæ:—

$$
\begin{aligned}
A_9 &= \tfrac{1}{9}(y_{10°}-y_{30°}+y_{50°}-y_{70°}+y_{90°}-y_{110°}+y_{130°}-y_{150°}+y_{170°});\\
B_9 &= \tfrac{1}{9}(y_{0°}-y_{20°}+y_{40°}-y_{60°}+y_{80°}-y_{100°}+y_{120°}-y_{140°}+y_{160°});\\
A_7 &= \tfrac{1}{7}(y_{12\cdot84}-y_{38\cdot56}+y_{64\cdot28}-y_{90°}+y_{115\cdot71}-y_{141\cdot43}+y_{167\cdot14});\\
B_7 &= \tfrac{1}{7}(y_{0°}-y_{25\cdot71}+y_{51\cdot43}-y_{77\cdot14}+y_{102\cdot85}-y_{128\cdot55}+y_{154\cdot28});\\
A_5 &= \tfrac{1}{5}(y_{18°}-y_{54°}+y_{90°}-y_{126°}+y_{162°});\\
B_5 &= \tfrac{1}{5}(y_{0°}-y_{36°}+y_{72°}-y_{108°}+y_{144°});\\
A_3 &= \tfrac{1}{3}(y_{30°}-y_{90°}+y_{150°})+A_9;\\
B_3 &= \tfrac{1}{3}(y_{0°}-y_{60°}+y_{120°})-B_9;\\
A_1 &= y_{90°}+A_3-A_5+A_7-A_9;\\
B_1 &= y_{0°}-B_3-B_5-B_7-B_9.
\end{aligned}
$$

SCHEDULE III.

HARMONIC ANALYSIS BY AVERAGING OF SELECTED ORDINATES.

(Harmonics above the 7th order assumed absent.)

First graduate the base-line of one period to read in degrees and then measure off ordinates as required in following formulæ:—

$$\begin{aligned}
A_7 &= \tfrac{1}{14}(y_{12\cdot84} - y_{38\cdot56} + y_{64\cdot28} - y_{90} + y_{115\cdot71} - y_{141\cdot43} + y_{167\cdot14} - y_{192\cdot85} + y_{218\cdot56} \\
&\qquad - y_{244\cdot28} + y_{270} - y_{295\cdot71} + y_{321\cdot43} - y_{347\cdot14}) ; \\
B_7 &= \tfrac{1}{14}(y_0 - y_{25\cdot71} + y_{51\cdot43} - y_{77\cdot14} + y_{102\cdot85} - y_{128\cdot56} + y_{154\cdot28} - y_{180} + y_{205\cdot71} \\
&\qquad - y_{231\cdot43} + y_{257\cdot14} - y_{282\cdot85} + y_{308\cdot56} - y_{334\cdot28}) \\
A_6 &= \tfrac{1}{12}(y_{15} - y_{45} + y_{75} - y_{105} + y_{135} - y_{165} + y_{195} - y_{225} + y_{255} - y_{285} + y_{315} - y_{345}) ; \\
B_6 &= \tfrac{1}{12}(y_0 - y_{30} + y_{60} - y_{90} + y_{120} - y_{150} + y_{180} - y_{210} + y_{240} - y_{270} + y_{300} - y_{330}) ; \\
A_5 &= \tfrac{1}{10}(y_{18} - y_{54} + y_{90} - y_{126} + y_{162} - y_{198} + y_{234} - y_{270} + y_{306} - y_{342}) ; \\
B_5 &= \tfrac{1}{10}(y_0 - y_{36} + y_{72} - y_{108} + y_{144} - y_{180} + y_{216} - y_{252} + y_{288} - y_{324}) ; \\
A_4 &= \tfrac{1}{8}(y_{22\cdot5} - y_{67\cdot5} + y_{112\cdot5} - y_{157\cdot5} + y_{202\cdot5} - y_{247\cdot5} + y_{292\cdot5} - y_{337\cdot5}) ; \\
B_4 &= \tfrac{1}{8}(y_0 - y_{45} + y_{90} - y_{135} + y_{180} - y_{225} + y_{270} - y_{315}) ; \\
A_3 &= \tfrac{1}{6}(y_{30} - y_{90} + y_{150} - y_{210} + y_{270} - y_{330}) ; \\
B_3 &= \tfrac{1}{6}(y_0 - y_{60} + y_{120} - y_{180} + y_{240} - y_{300}) ; \\
A_2 &= \tfrac{1}{4}(y_{45} - y_{135} + y_{225} - y_{315}) + A_6 ; \\
B_2 &= \tfrac{1}{4}(y_0 - y_{90} + y_{180} - y_{270}) - B_6 ; \\
A_1 &= \tfrac{1}{2}(y_{90} - y_{270}) + A_3 - A_5 + A_7 ; \\
B_1 &= \tfrac{1}{2}(y_0 - y_{180}) - B_3 - B_5 - B_7 ; \\
A_0 &= y_0 - B_1 - B_2 - B_3 - B_4 - B_5 - B_6 - B_7 .
\end{aligned}$$

* ABSTRACT.

Assume with Fourier that the curve representing any periodic single-valued function of x may be expressed by the harmonic series ;

$$\begin{aligned} y = {} & A_1 \sin x + A_2 \sin 2x + A_3 \sin 3x + \dots\dots \\ & + B_1 \cos x + B_2 \cos 2x + B_3 \cos 3x. \end{aligned}$$

Then to find the coefficient of any term, A_n or B_n, it suffices—subject to a limitation stated below—to measure off on the curve $2n$ equidistant ordinates over one period, that is, spaced at successive intervals apart of π/n. Then, having reversed the sign of every alternate ordinate, the simple algebraic mean of them gives the coefficient sought. For cosine-coefficients the first ordinate must be taken at the origin ; while for sine-coefficients the first ordinate must be taken at a point $\frac{1}{2}\frac{\pi}{n}$ from the origin. In symbols this is

$$A_n = \frac{1}{2n}\left\{ y_{\frac{1}{2}\pi/n} - y_{\frac{3}{2}\pi/n} + y_{\frac{5}{2}\pi/n} - y_{\frac{7}{2}\pi/n} + \dots\dots - y_{\frac{4n-1}{2}\pi/n} \right\} ;$$

$$B_n = \frac{1}{2n}\left\{ y_0 \quad - y_{\pi/n} + y_{2\pi/n} - y_{3\pi/n} + \dots\dots - y_{(2n-1)\pi/n} \right\}.$$

The limitation is that if, in finding the nth harmonic, there be present also the $3n$th, or the $5n$th, or the $7n$th, &c., these will be included in the average found by the process, and must, if present, be separately determined and eliminated.

The process is much facilitated by the use of templates of transparent celluloid having equispaced vertical lines engraved upon them. They are laid down on the curve, and the values of the selected ordinates are thus readily measured off. For analysis of valve-motions, of alternating-current curves, of tidal observations, and diurnal magnetic variations, the method presents certain advantages, as it requires no multiplication of ordinates by sines or cosines.

DISCUSSION.

The **President** commented upon the extreme ease with which the method adapted itself to the analysis of experimental curves.

Dr. **Russell** remarked that the method can be usefully employed in interpolation as well as in harmonic analysis. In this connection he gave a brief résumé of Gauss's extension of Lagrange's interpolation formula ("Werke," Vol. III., p. 281). He also pointed out certain cases in which Fourier's analysis fails to be of practical use.

Sir G. H. **Darwin** communicated the following remarks: In 1874 Mr. Archibald Smith published, in the Admiralty Scientific Manual on "Deviations of the Compass," schedules whereby the multiplications in the usually accepted process are simple. This same arrangement has been in use for the harmonic analysis of tides since 1870, and in his "Manual of Tidal Observations," Major Baird gave multiplication tables for the sines of 15, 45, 60, 75 degs., which render this part of the work so easy that avoidance of all multiplication hardly seems necessary. In 1884, in "Tables and Formulæ to Facilitate the Computation of Harmonic Coefficients," General Sir Richard Strachey gave rules which reduce the multiplications still further. In both these schemes Prof. Thompson's principle is used for some of the higher harmonics. Smith's and Strachey's schedules are arranged for 24 hourly values, but they may be equally well used for 12 ordinates. Schedules for the use of any other number of ordinates would have to be prepared specially, and in such cases Prof. Thompson's rules might prove very convenient. I have not worked out the probable errors in the several methods, but it seems likely that they would be smaller in Smith's method than in any other. Examples of both these schedules will be found in Vol. I. of my "Collected Papers," or in the report to the Brit. Assoc. on Harmonic Analysis (1883), and to article "Tides," in the "Admiralty Scientific Manual" (1886).

XXXII. *Short Table of Circular and Hyperbolic Functions for Complex Values of the Argument.* By A. JOHNSTONE, *B.Sc.*

COMMUNICATED BY PROF. C. H. LEES, F.R.S. RECEIVED MAY 12, 1911.

IN view of the recent prominence given to the circular and hyperbolic functions for complex values of the argument by the use of them in calculations regarding telegraphic and telephonic circuits, I have, at the suggestion of Prof. Lees, calculated out the following table in which the values of the cosh and sinh functions will be found.

In order to make the table more generally available the following formulæ, connecting the values of the functions for different values of the argument, are given :—

Values of Cosh $(x+iy)=R+iU$.

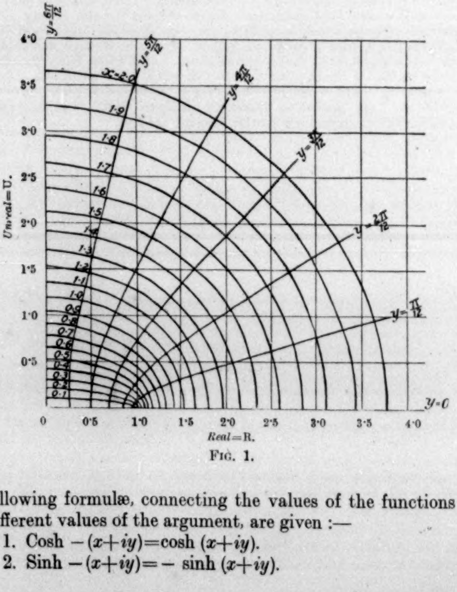

FIG. 1.

1. Cosh $-(x+iy)=\cosh(x+iy)$.
2. Sinh $-(x+iy)=-\sinh(x+iy)$.

Cosh $(x+iy)$.

Values of x.	Values of y.													
	0		15° = $\frac{\pi}{12}$ = 0·2618		30° = $\frac{2\pi}{12}$ = 0·5236		45° = $\frac{3\pi}{12}$ = 0·7854		60° = $\frac{4\pi}{12}$ = 1·0472		75° = $\frac{5\pi}{12}$ = 1·3090		90° = $\frac{6\pi}{12}$ = 1·5708	
	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal
	R	U	R	U	R	U	R	U	R	U	R	U	R	U
0·0	1·0000	0	0·9659	0	0·8660	0	0·7071	0	0·5000	0	0·2588	0	0	0
0·1	1·0050	…	0·9708	0·0259	0·8704	0·0501	0·7106	0·0709	0·5025	0·0868	0·2601	0·0968	…	0·1002
0·2	1·0201	…	0·9853	0·0521	0·8834	0·1007	0·7213	0·1423	0·5100	0·1743	0·2640	0·1944	…	0·2013
0·3	1·0453	…	1·0097	0·0788	0·9053	0·1523	0·7391	0·2153	0·5227	0·2637	0·2705	0·2941	…	0·3045
0·4	1·0811	…	1·0443	0·1063	0·9363	0·2054	0·7645	0·2905	0·5405	0·3558	0·2798	0·3968	…	0·4108
0·5	1·1276	…	1·0892	0·1349	0·9765	0·2606	0·7973	0·3685	0·5638	0·4513	0·2918	0·5033	…	0·5211
0·6	1·1855	…	1·1451	0·1648	1·0267	0·3183	0·8383	0·4502	0·5927	0·5514	0·3068	0·6150	…	0·6367
0·7	1·2552	…	1·2124	0·1963	1·0870	0·3793	0·8876	0·5364	0·6276	0·6570	0·3249	0·7328	…	0·7586
0·8	1·3374	…	1·2918	0·2299	1·1582	0·4441	0·9457	0·6280	0·6687	0·7691	0·3461	0·8578	…	0·8881
0·9	1·4331	…	1·3843	0·2657	1·2411	0·5133	1·0134	0·7259	0·7165	0·8890	0·3709	0·9915	…	1·0265
1·0	1·5431	…	1·4905	0·3042	1·3364	0·5876	1·0911	0·8310	0·7715	1·0178	0·3994	1·1352	…	1·1752
1·1	1·6685	…	1·6116	0·3457	1·4450	0·6678	1·1798	0·9445	0·8343	1·1568	0·4318	1·2902	…	1·3357
1·2	1·8107	…	1·7490	0·3907	1·5681	0·7547	1·2804	1·0674	0·9053	1·3073	0·4686	1·4581	…	1·5095
1·3	1·9709	…	1·9037	0·4396	1·7068	0·8492	1·3936	1·2010	0·9855	1·4709	0·5101	1·6405	…	1·6984
1·4	2·1509	…	2·0776	0·4929	1·8627	0·9522	1·5209	1·3465	1·0755	1·6492	0·5567	1·8394	…	1·9043
1·5	2·3524	…	2·2722	0·5511	2·0372	1·0646	1·6634	1·5056	1·1762	1·8440	0·6089	2·0567	…	2·1293
1·6	2·5775	…	2·4897	0·6149	2·2322	1·1878	1·8226	1·6798	1·2887	2·0573	0·6671	2·2947	…	2·3756
1·7	2·8283	…	2·7319	0·6847	2·4494	1·3228	1·9999	1·8707	1·4142	2·2912	0·7320	2·5555	…	2·6456
1·8	3·1075	…	3·0016	0·7615	2·6912	1·4711	2·1973	2·0804	1·5537	2·5480	0·8043	2·8419	…	2·9422
1·9	3·4177	…	3·3012	0·8459	2·9598	1·6341	2·4167	2·3110	1·7089	2·8303	0·8846	3·1568	…	3·2682
2·0	3·7622	…	3·6340	0·9387	3·2582	1·8134	2·6603	2·5646	1·8811	3·1410	0·9737	3·5033	…	3·6269
2·1	4·1443	…	4·0031	1·0409	3·5891	2·0109	2·9305	2·8439	2·0722	3·4831	1·0726	3·8849	…	4·0219
2·2	4·5679	…	4·4123	1·1536	3·9559	2·2286	3·2300	3·1516	2·2840	3·8600	1·1823	4·3052	…	4·4571
2·3	5·0372	…	4·8656	1·2778	4·3623	2·4685	3·5618	3·4910	2·5186	4·2756	1·3037	4·7688	…	4·9370
2·4	5·5570	…	5·3677	1·4148	4·8125	2·7331	3·9294	3·8652	2·7785	4·7339	1·4383	5·2799	…	5·4662
	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′
	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real
	90° = $\frac{6\pi}{12}$ = 1·5708		75° = $\frac{5\pi}{12}$ = 1·3090		60° = $\frac{4\pi}{12}$ = 1·0472		45° = $\frac{3\pi}{12}$ = 0·7854		30° = $\frac{2\pi}{12}$ = 0·5236		15° = $\frac{\pi}{12}$ = 0·2618		0	
	Values of y.													

Sinh $(x+iy)$.

Cosh $(x+iy)$. – *Continued.*

Values of x	Values of y.													
	0		15° = $\frac{\pi}{12}$ = 0·2618		30° = $\frac{2\pi}{12}$ = 0·5236		45° = $\frac{3\pi}{12}$ = 0·7854		60° = $\frac{4\pi}{12}$ = 1·0472		75° = $\frac{5\pi}{12}$ = 1·3090		90° = $\frac{6\pi}{12}$ = 1·5708	
	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal
	R	U	R	U	R	U	R	U	R	U	R	U	R	U
2·5	6·1323	0	5·9233	1·5659	5·3107	3·0251	4·3362	4·2781	3·0661	5·2396	1·5872	5·8440	0	6·0502
2·6	6·7690	…	6·5384	1·7327	5·8621	3·3474	4·7864	4·7339	3·3845	5·7978	1·7519	6·4666	…	6·6947
2·7	7·4735	…	7·2188	1·9169	6·4722	3·7031	5·2346	5·2370	3·7367	6·4140	1·9343	7·1539	…	7·4063
2·8	8·2527	…	7·9715	2·1202	7·1470	4·0960	5·8355	5·7925	4·1264	7·0944	2·1360	7·9128	…	8·1919
2·9	9·1146	…	8·8040	2·3448	7·8935	4·5298	6·4450	6·4061	4·5573	7·8458	2·3590	8·7509	…	9·0596
3·0	10·068	…	9·7247	2·5928	8·7189	5·0089	7·1189	7·0837	5·0339	8·6758	2·6057	9·6765	…	10·018
3·2	12·287	…	11·868	3·1695	10·641	6·1230	8·6879	8·6592	6·1433	10·605	3·1800	11·829	…	12·246
3·4	14·999	…	14·488	3·8733	12·989	7·4827	10·606	10·582	7·4994	12·960	3·8819	14·455	…	14·965
3·6	18·313	…	17·689	4·7326	15·859	9·1428	12·949	12·930	9·1564	15·836	4·7397	17·662	…	18·286
3·8	22·362	…	21·600	5·7819	19·366	11·170	15·812	15·796	11·181	19·346	5·7877	21·578	…	22·339
4·0	27·308	…	26·378	7·0631	23·650	13·645	19·310	19·297	13·654	23·634	7·679	26·360	…	27·290
4·5	45·014	…	43·480	11·648	38·983	22·502	31·830	31·822	22·507	38·974	11·651	43·470	…	45·003
5·0	74·210	…	71·681	19·205	64·268	37·102	52·474	52·470	37·105	64·262	19·207	71·675	…	74·203
5·5	122·35	…	118·18	31·665	105·96	61·172	86·513	86·510	61·174	105·95	31·666	118·18	…	122·34
6·0	201·72	…	194·84	52·207	174·69	100·86	142·63	142·63	100·86	174·69	52·208	194·84	…	201·71
6·5	332·57	…	321·24	86·076	288·01	166·29	235·16	235·16	166·29	288·01	86·076	321·24	…	332·57
7·0	548·32	…	529·63	141·91	474·86	274·16	387·72	387·72	274·16	474·86	141·91	529·63	…	548·32
8·0	1490·5	…	1439·7	385·76	1290·8	745·24	1053·9	1053·9	745·24	1290·8	385·76	1439·7	…	1490·5
9·0	4051·5	…	3913·5	1048·6	3508·7	2025·8	2864·9	2864·9	2025·8	3508·7	1048·6	3913·5	…	4051·5
10·0	11013	…	10638	2850·4	9537·7	5506·6	7787·5	7787·5	5506·6	9537·7	2850·4	10638	…	11013
Approaches x Large	$e^{x}/2$	…	0·9659$e^{x}/2$	0·2588$e^{x}/2$	0·8660$e^{x}/2$	0·5000$e^{x}/2$	0·7071$e^{x}/2$	0·7071$e^{x}/2$	0·5000$e^{x}/2$	0·8660$e^{x}/2$	0·2588$e^{x}/2$	0·9659$e^{x}/2$	…	$e^{x}/2$
	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′	U′	R′
	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real	Unreal	Real
	90° = $\frac{6\pi}{12}$ = 1·5708		75° = $\frac{5\pi}{12}$ = 1·3090		60° = $\frac{4\pi}{12}$ = 1·0472		45° = $\frac{3\pi}{12}$ = 0·7854		30° = $\frac{2\pi}{12}$ = 0·5236		15° = $\frac{\pi}{12}$ = 0·2618		0	
	Values of y.													

Sinh $(x+iy)$.

3. $\cosh (x \pm iy) = R \pm iU.$
4. $\sinh (x \pm iy) = R' \pm iU'.$
5. $\cosh (x+iy+2n\pi i) = \cosh (x+iy),$ n integral.
6. $\sinh (x+iy+2n\pi i) = \sinh (x+iy).$
7. $\cosh (x+iy+\pi i) = -\cosh (x+iy).$
8. $\sinh (x+iy+\pi i) = -\sinh (x+iy).$
9. $\cosh \left(x+iy+\frac{\pi}{2}i \right) = i \sinh (x+iy).$
10. $\sinh \left(x+iy+\frac{\pi}{2}i \right) = i \cosh (x+iy).$
11. $\cos (x+iy) = \cosh i(x+iy) = \cosh (ix-y) = \cosh (y-ix)$
12. $\sin (x+iy) = -i \sinh i(x+iy) = -i \sinh (ix-y) = i \sinh (y-ix).$

EXPLANATION OF THE TABLE.

To find the value of $\frac{\cosh}{\sinh}(x+iy)$, reduce y to angular measure by multiplying by $\frac{180}{\pi}$. Subtract from its value any multiple of 360 deg. (*see* equations 5 or 6 above). If the difference is less than 90 deg., use the table $\frac{\cosh}{\sinh}(x+iy)$. If is greater than 90 deg. but less than 180 deg., subtract 90 deg. and use the table $\frac{\sinh}{\cosh}(x+iy)$ (*see* equations 9 or 10 above). If it is greater than 180 deg., subtract 180 deg. and proceed as before (*see* equations 7 or 8 above).

Interpolation for values of x greater than 1 will be facilitated by Fig. 1.

By the aid of equations 11 and 12 above, $\frac{\cos}{\sin}(x+iy)$ may be found in the same way, the value of x being expressed in angular measure.

XXXIII. *On the Measurement of Contact Differences of Potential.*

By Prof. A. ANDERSON, *M.A., and* J. E. BOWEN, *M.A.*

RECEIVED MAY 15, 1911.

THE following methods of determining the Volta, or contact, differences of potential of pairs of different metals are the outcome of the overcoming of some difficulties that arose in working with a Dolezalek quadrant electrometer. The values obtained, though correctly measured to the third decimal place, lay no claim to any scientific importance, inasmuch as they, in common with results obtained by other methods, depend so greatly on the state of the surfaces at the time of measurement. In several cases the changes with time after the metals were cleaned were considerable, and very notably so in the case of aluminium.

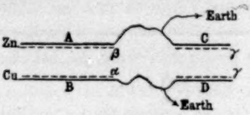

FIG. 1.

The following statement of the theory usually adopted will make the methods clear :—

The layer of air in contact with a metal in chemical equilibrium with it is at a potential different from that of the metal, the difference varying for different metals and for different states of the surface of the same metal.

In Fig. 1, A represents a plate of zinc and B a plate of copper. C and D represent the two pairs of quadrants of the electrometer. If, as in the figure, A, B, C and D are all connected to earth, the potential of the layer of air in contact with A will be, say, β, that of the layer of air in contact with B, a, and that of the air in contact with both pairs of quadrants, γ. There is thus a field of force in the air between A and B, but no field of force in the air between the pairs of quadrants.

If, now, A and C are insulated, and an ionising source be employed to destroy the field between A and B, we shall have, when equilibrium is attained, the state of affairs represented in Fig. 2.

The flow of negative electrons and positive atoms has reduced the potential of A to $-(\beta-a)$. The potential of the air in contact with one pair of quadrants is $-(\beta-a)+\gamma$, and that of the air in contact with the other, γ.

The electrometer then measures $\beta-a$, which is what is known as the contact difference of potential. This is the first, or deflection, method.

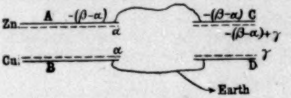

FIG. 2.

The second method is a null, or compensation, method, and will be easily understood from Fig. 3.

The copper plate B is insulated from its pair of quadrants which are kept earthed, the air between A and B ionised, and a positive potential, V, put on to the copper plate, such that, when the earth connection of the zinc plate and its pair of quadrants is broken, there is no deflection of the electrometer needle. We must then have

$$V+a=\beta, \text{ or } V=\beta-a.$$

The apparatus and its arrangement will be readily understood from Fig. 4.

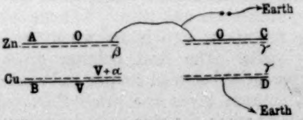

FIG. 3.

E is a copper cylinder resting on a block of paraffin, P, and furnished with a copper lid, L. It was, in the first method, earthed, but in the second method it was insulated and potentials applied to it by means of a Nalder's potentiometer reading to one-thousandth of a volt. B is a copper plate resting on the bottom of the cylinder. This plate is unnecessary as the bottom of the cylinder will serve the same purpose. A is a plate of zinc or other metal, the contact P.D. between which and copper is to be found. It is supported by a ring of paraffin wax within which, resting on the plate B, was the ionising agent, a sealed glass tube containing radium bromide. The copper wire connecting A to the electrometer passed through an earthed copper tube, T, filled with paraffin, this tube starting from within another copper tube, S, forming part of the cylinder, and ending within the earthed metallic cage containing the electrometer. H is a mercury cup used to break connection between A and the earth. The deflection of the electrometer was 405 mm. for 1 volt.

The results obtained by the two methods agreed very closely, and did not differ by more than two or three units in the third decimal place.

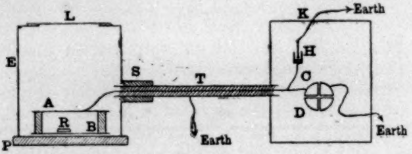

FIG. 4.

To test the addition law a zinc cylinder was used instead of a copper one. The tube T was zinc, and it was found necessary to use a zinc strip instead of a copper wire to connect A with the electrometer.

The following are the results obtained in an experiment in which the second method was used. The metals were cleaned and left aside for about two hours when the measurements were rapidly made. The first column gives the contact differences of potential between copper and nine other metals.

The second column gives the same P.D. calculated from observations made when the zinc cylinder was used, assuming the truth of the addition law.

	Volts.	Volts.
Al	0·824	0·831
Zn	0·762	0·763
Pb	0·550	0·549
Sn	0·472	0·476
Fe	0·194	0·191
Brass	0·154	0·152
Au	−0·115	−0·113
Ag	−0·132	−0·135
Pt	−0·200	−0·198

POSTCRIPT.

[MAY 31, 1911.]

The results obtained by the two methods were carefully compared and very many measurements were made. At first, the results obtained by the deflection method were always less than those obtained by the null method, but, after some difficulties connected principally with leakage had been overcome, the results were, in all cases, practically identical. All the metals were freshly scratched before measurements were made except the gold, silver and platinum, which were carefully cleaned to remove grease. After fresh scratching the P.D. changed slightly in all cases, but soon came to a practically steady state. In the case of copper-lead, the P.D. increased; in that of copper-aluminium the change, which was a decrease, was more marked. There is no doubt that both of these changes were due principally to the lead and aluminium themselves.

In an experiment recently made the P.D. between a freshly scratched aluminium plate and a copper plate which had been left untouched for over a fortnight was measured. At first the P.D. in volts, copper being positive to aluminium, was 1·062, in 5 minutes it decreased to 1·008, in 10 minutes to 0·984, in 25 minutes to 0·952, in 50 minutes to 0·94, and in 60 minutes to 0·938, after which the rate of change was very slow.

The copper plate was then compared with another copper plate freshly scratched, when the former was found to be positive to the latter, the difference being 0·12, which diminished very slowly, the change being 0·004 in 30 minutes.

It will be seen that the deflection method is particularly well adapted for showing alterations in the contact difference due to lapse of time, or to other changes in the conditions which may be controlled.

ADDENDUM.

[RECEIVED JUNE 7, 1911.]

An opportunity of applying the deflection method presented itself quite recently. A Paper published in "Comptes Rendus" (March 13, 1911) gives some results of a research by M. de Broglie on the lowering of the apparent contact differences of potential by the removal of the adhering layers of moisture. The author concludes that almost the whole of the contact

P.D.s is due to layers of moisture on the plates. We thought it might be worth while to test this by the deflection method.

A zinc plate was insulated inside the copper cylinder, whose capacity is about 1,500 c.c., the surfaces of both metals having been thoroughly scratched with emery paper. The contact potential shown was 0·734 volt. A current of air, dried by being passed through strong sulphuric acid and calcium chloride, and heated during its passage through a glass tube raised to a high temperature, was then pumped through the vessel, which had previously been made as air-tight as possible. The pump was worked for four hours.

The P.D. rose gradually for about an hour, when it stood at 0·796 volt. Part of this, however, was certainly due to a frictional negative charge produced on the zinc plate by the current of dry air flowing past it. When the pump was stopped for a minute the deflection diminished by about eight divisions, corresponding to a decrease of 0·019 volt. The apparent increase of P.D. due to the frictional charge was greater the higher the temperature to which the air was raised, and the more vigorously the pump was worked. The P.D. may also have been influenced by the rise in temperature of the plates, but this effect, if it existed, was small.

During the second hour the P.D. gradually fell to 0·767 volt, and remained at this, with exceedingly small up and down variations, for the remaining two hours, so that it could not be said with certainty that for these two hours there was a change of one-thousandth of a volt. The remarkable thing about it was its steadiness.

At the end of the four hours the pump was stopped, when the P.D. went down quickly to, and remained at, 0·722 volt. The result of four hours' continuous drying was, therefore, a fall of 0·012 volt. If the P.D. can be brought down to a few hundredths of a volt a much more effective method of drying than that used in this experiment would be necessary. Indeed, the experiment gave one the impression that the current of drying air, so far from having the effect of lowering the P.D., actually kept it steady.

It must be noted that M. de Broglie's plates were heated to temperatures varying between 100°C. and 400°C., and then allowed to cool in an enclosure 1 cubic metre in volume containing air kept dry by chloride of calcium.

We are engaged in further experiments with a view to a more complete test of M. de Broglie's interesting result.

ABSTRACT.

The Paper describes two methods of measuring the contact differences of potential of pairs of metals. The first, or deflection, method depends on the property which a radioactive source has of destroying a field of electrostatic force in air, and the second, or null, method on the possibility of determining by means of such a source whether such a field exists between two plates at zero potential. Measurements were made on 10 different metals, and it was found that both methods gave practically the same results provided that the time which was allowed to elapse between the two measurements was sufficiently small. The addition law was verified.

XXXIV. *The Capacity Coefficients of Spherical Electrodes. By* ALEXANDER RUSSELL, *M.A., D.Sc., M.I.E.E.*

RECEIVED MAY 23, 1911. READ JUNE 30, 1911.

IN practical electrical work a knowledge of the values of the capacity coefficients of spheres is sometimes very useful. Considering the importance of these coefficients it is remarkable that in no book with which the author is acquainted is there a numerical example given of their values when the spheres are unequal. The probable explanation is that physicists have found the ordinary formulæ unsuitable for computation. The author, having recently had to calculate their values in many cases, has been led to modify the old formulæ and to invent new ones with the object of diminishing the numerical labour involved.

The formulæ given below solve the problem completely. The labour entailed in computing by their means being, even in the worst cases, very little greater than that required to find the angles of a triangle when the sides are given.

Maxwell's Formulæ.

Let the radii of the two spherical conductors be a and b respectively and let c be the distance between their centres. If the charges and potentials of the spheres be q_1, q_2 and v_1, v_2 respectively, we have

$$q_1 = k_{1,1}v_1 + k_{1,2}v_2 \text{ and } q_2 = k_{2,2}v_2 + k_{1,2}v_1, \quad . \quad . \quad . \quad (1)$$

where $k_{1,1}$, $k_{1,2}$ and $k_{2,2}$ are the capacity coefficients of the two spheres, the values of which have to be computed from the known valves of a, b and c. Maxwell's solution,* slightly modified so as to make it more symmetrical, is given by the following equations:—

$$k_{1,1} = \lambda\Sigma\frac{1}{\sinh(\alpha_1 + s\omega_1)} \text{ and } -k_{1,2} = \lambda\Sigma\frac{1}{\sinh(s+1)\omega_1}, \quad (2)$$

where $4c^2\lambda^2 = (c+a+b)(c-a-b)(c+a-b)(c-a+b)$. (3

and $\sinh\alpha_1 = \lambda/a$, $\sinh\omega_1 = \lambda c/ab$. . . . . (4)

The summation is taken from $s=0$ to $s=$infinity, and the value of $k_{2,2}$ is found by writing β_1 for α_1 in the formula for $k_{1,1}$, where $\sinh\beta_1 = \lambda/b$. It is easy to show that λ is the radius of the sphere which cuts the two given spheres orthogonally.

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* *Cf.* Maxwell, Vol. I., §173.

When they are close together λ is very small and, therefore, a_1, β_1 and ω_1 are also very small, and so the labour involved in computing by these formulæ is prohibitive.

Kirchhoff's and Poisson's Formulæ.

If in Maxwell's formulæ we write $\epsilon^{a_1}=1/\xi$, $\epsilon^{\beta_1}=1/\eta$ and $\epsilon \;=1/a$, we get

$$k_{11}=2\lambda\left\{\frac{\xi}{1-\xi^2}+\frac{a\xi}{1-a^2\xi^2}+\frac{a^2\xi}{1-a^4\xi^2}+\ldots\ldots\right\} \quad . \quad . \quad (5)$$

and $$-k_{12}=2\lambda\left\{\frac{a}{1-a^2}+\frac{a^2}{1-a^4}+\frac{a^3}{1-a^6}+\ldots\ldots\right\} \quad . \quad . \quad (6)$$

The formula for $k_{2,2}$ is found by writing η for ξ in (5). It may be shown that

$$\xi=(1+\lambda^2/a^2)_{1/2}-\lambda/a\;;\; a=(c\xi-a)/b, \text{ and } \eta=a/\xi. \quad (7)$$

The last equation follows since $\omega_1=a_1+\beta_1$. The formulæ were practically given in this shape by Kirchhoff.* An admirably concise proof of them by Kelvin's method of images is given in Jeans' "Electricity and Magnetism," p. 192. Jeans also proves that they are equivalent to the definite integral solutions given by Poisson.†

When ξ and η are nearly unity the numerical labour is very heavy. To get over this difficulty Kirchhoff‡ modified the series so as to make them converge more rapidly by means of the following interesting extension of Clausen's theorem—

$$\frac{1}{1-a}+\frac{\beta}{1-a\gamma}+\frac{\beta^2}{1-a\gamma^2}+\ldots=\Sigma a^n\beta^n\gamma^{n^2}\frac{1-a\beta\gamma^{2n}}{(1-a\gamma^n)(1-\beta\gamma^n)}$$

the summation being taken from $n=0$ to $n=$infinity.

The methods given below, however, are better.

Barnes's Solution.

In the "Quarterly Journal," No. 138, 1903, p. 155, Barnes shows that the capacity coefficients can be determined in terms of double Gamma-functions. Until these functions are tabulated, however, the solutions given are no help to the physicist.

The Author's Formulæ.

The author has found the following modification of Kirchhoff's formulæ very convenient when making numerical calculations. Let $2\lambda R_n$ denote the sum after the first n terms of the series for $k_{1,1}$. Then

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* "**Gesammelte Abhandlungen,**" **p. 78.**

† "**Memoires de l'Institut,**" **1812.**

‡ "**Annalen der Physik,**" **Vol. XXVII., p. 673, 1886.**

$$R_n = \frac{\xi a^n}{1-\xi^2 a^{2n}} + \frac{\xi a^{n+1}}{1-\xi^2 a^{2n+2}} + \ldots\ldots,$$

and hence, by grouping the terms differently, we have

$$R_n = \frac{\xi a^n}{1-a} + \frac{(\xi a^n)^3}{1-a^3} + \frac{(\xi a^n)^5}{1-a^5} + \ldots\ldots \quad . \quad . \quad . \quad (8)$$

We see that when ξa^n is small the series (8) converges very rapidly.

Similarly, if $2\lambda R'_n$ be the remainder after n terms of the series for $-k_{1,2}$, we have

$$R'_n = \frac{a^{n+1}}{1-a} + \frac{a^{3n+3}}{1-a^3} + \frac{a^{5n+5}}{1-a^5} + \ldots\ldots \quad . \quad . \quad . \quad . \quad (9)$$

Using this method it will be found that we can readily compute the values of the coefficients even when the distance between the spheres is only the hundredth part of the radius of the smaller sphere.

In the particular case when n is zero, the formula for $k_{1,1}$ becomes

$$k_{1,1} = 2\lambda\left\{\frac{\xi}{1-a} + \frac{\xi^3}{1-a^3} + \frac{\xi^5}{1-a^5} + \ldots\ldots\right\} . \quad . \quad . \quad (10)$$

Equally simple formulæ for $-k_{1,2}$ and $k_{2,2}$ are obtained by writing a and η respectively for ξ in (10).

When $n=1$, the formulæ become

$$k_{1,1} = a + 2\lambda\xi\left\{\frac{a}{1-a} + \frac{a^3}{1-a^3}\xi^2 + \frac{a}{1-a^5}\xi^4 + \ldots\ldots\right\} . \quad (11)$$

and $$-k_{1.2} = \frac{ab}{c} + 2\lambda a\left\{\frac{a}{1-a} + \frac{a^3}{1-a^3}a^2 + \frac{a^5}{1-a^5}a^4 + \ldots\ldots\right\} . \quad (12)$$

Let us suppose, for example, that $a=7r$, $b=r$ and $c=10r$. Then, by (3) and (7)

$$\lambda = 2{\cdot}4r, \quad \xi = 5/7, \quad \eta = 1/5 \quad \text{and} \quad a = 1/7.$$

Hence, by (11) and (12),

$$k_{1,1} = 7r + \frac{4}{7}r\{1 + 0{\cdot}0089509 + 0{\cdot}0000929 + 0{\cdot}0000009 + \ldots\ldots$$
$$= 7{\cdot}5765970r.$$
$$-k_{1.2} = 0{\cdot}7r + (8/70)\{1 + 0{\cdot}0003580 + 0{\cdot}0000001 + \ldots\ldots\}$$
$$= 0{\cdot}8143266r.$$

Similarly

$$k_{2,2} = 1{\cdot}1601124r.$$

As a further example let us suppose that $a=98$, $b=10{\cdot}8$ and $c=a+b+0{\cdot}2=109$. In this case, taking $n=4$, we find that only seven terms of the series for R_4 need to be computed in order to find that

$$k_{1,1}=1{\cdot}2708614a.$$

When the distance between the spheres is, as in the last example, only a small fraction of the radius of the smaller one, the computation of the coefficients by the above method is laborious. In this case it saves labour to use the formulæ* which the author has previously given. The closer the spheres are to one another the simpler these formulæ become. We shall write for the function F (x)† previously used, $-\psi(1/x)-\log x$, where $\psi(x)$ is defined by

$$\psi(x)=\frac{\delta}{\delta x}\{\log\Gamma(x)\}.$$

This function has been tabulated by Gauss ‡ The formulæ now become

$$\frac{k_{1,1}}{2\lambda}=-\frac{\psi(a_1/\omega_1)+\log(\omega_1/2)}{2\omega_1}+\frac{\omega_1}{24}\left(\frac{1}{6}-\frac{a_1\beta_1}{\omega_1^2}\right)$$
$$+\frac{7\omega_1^3}{2880}\left(\frac{1}{30}-\frac{a_1^2\beta_1^2}{\omega_1^4}\right)+\ldots\ldots \quad . \quad . \quad (13)$$

$$-\frac{k_{1,2}}{2\lambda}=\frac{\gamma-\log(\omega_1/2)}{2\omega_1}+\frac{\omega_1}{144}+\frac{7\omega_1^3}{86400}+\ldots\ldots \quad . \quad . \quad . \quad (14)$$

$$\frac{k_{2,2}}{\lambda}=\frac{k_{1,1}}{\lambda}-\frac{\pi}{\omega_1}\cot\frac{\pi a_1}{\omega_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where $\gamma=0{\cdot}577216\ldots\ldots=$ Euler's constant. The function $\psi(x)$ is easily computed by the series

$$\psi(x)=-\frac{1}{x}-\gamma+\frac{x}{1+x}+(S_2-1)x-(S_3-1)x^2+(S_4-1)x^3-\ldots,(16)$$

where $S_m=\sum_1^\infty\frac{1}{n^m}$. The values of S_2, S_3,...... are given in Dale's "Mathematical Tables," p. 92.

If the distance x between the spheres be not greater than a tenth of the radius of the smaller sphere and an accuracy of the tenth part of 1 per cent. suffices the following formulæ are the best to use :—

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* "Proc." Royal Society, A, Vol. LXXXII., p. 527.

† *Loc. cit.*, p. 525.

‡ Werke, Bd. 3, pp. 161-162. *See also* "Functionentafeln," Jahnke and Emde, p. 30.

$$k_{1,1}=\frac{ab}{a+b}\left\{1+\frac{a^2+b^2-ab}{3ab(a+b)}x\right\}\left[-\psi\left\{\frac{b}{a+b}+\frac{x(a-b)}{3(a+b)^2}\right\}\right.$$
$$\left.+\tfrac{1}{2}\log\frac{2ab}{(a+b)x}+\frac{4(a-b)^2+ab}{36ab(a+b)}x\right], \quad (17)$$

$$-k_{1,2}=\frac{ab}{a+}\left\{1+\frac{a^2+b^2-ab}{3ab(a+b)}x\right\}\left\{\gamma+\tfrac{1}{2}\log\frac{2ab}{(a+b)x}\right.$$
$$\left.+\frac{4(a-b)^2+7ab}{36ab(a+b)}x\right\}. \quad . \quad (18)$$

The value of $k_{2,2}$ is found by interchanging a and b in (17).

Calculating by (17) when $a=98$, $b=10\cdot8$ and $c=109$, we get $k_{1,1}/a=1\cdot27070$. The true value found above is 1·27086. Similarly we find that $-k_{1,2}/a=0\cdot2861$ and $k_{2,2}/a=0\cdot3036$.

Formula for the Difference of the Self-coefficients.

From (15) we get

$$k_{1,1}-k_{2,2}=\frac{\pi\lambda}{\log(1/\alpha)}\cot\left\{\pi\frac{\log(1/\xi)}{\log(1/\alpha)}\right\}. \quad . \quad . \quad (19)$$

This is a very accurate formula when x is small. Even when x is as great as $3ab/(a+b)$ the maximum inaccuracy is only about 1 in 1,000.

For example, when $c=10r$, $a=7r$ and $b=r$, we find that $_{1,1}-k_{2,2}=6\cdot4175r$. The true value found above is $6\cdot4165r$.

Cases in which the Computation is easy.

If $a=l^2h^2-k^2m^2$, $b=l^2k^2-h^2m^2$ and $c=(h^2+k^2)(l^2+m^2)$, where l, h, k and m are any numbers which make a and b positive, then

$$\lambda=2hklm,\quad \xi=\frac{lh-km}{lh+km},\quad \eta=\frac{lk-hm}{lk+hm}\quad\text{and}\quad \alpha=\xi\eta.$$

Hence λ, ξ, η and α are rational and this simplifies the calculation.

For example, suppose that $h=k=l=3$ and that $m=1$. Then $\xi=\eta=1/2$, and $\alpha=1/4$. We also have $a=b=72$, $c=180$ and $\lambda=54$.

Hence, by (11),

$$k_{1,2}=k_{2,2}=72+108\left(\frac{1}{3\times2}+\frac{1}{63\times8}+\frac{1}{1023\times32}+\dots\right)$$
$$=90\cdot2176.$$

And also by (12),

$$-k_{1,2}=28\cdot8+108\left(\frac{1}{3\times4}+\frac{1}{63\times64}+\dots\right)$$
$$=37\cdot8269.$$

Another interesting set of cases is when either of the equations $\omega_1=3a_1$ or $\omega_1=4a_1$ is satisfied. In the former case $a^2=b(b+c)$ and in the latter $c^2b=(a+b)(a-b)^2$. In either case the calculation can be simplified very appreciably.

Systems of Spheres which have the same Mutual Coefficients and the same Orthogonal Sphere.

All the pairs of spheres for which the values of a, b and c satisfy the equations

$$ab\left\{1-\frac{(a-b)^2}{c^2}\right\}=\mathrm{R}^2 \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and

$$c-\frac{(a-b)^2}{c}=c_1 \quad . \quad . \quad . \quad . \quad . \quad (21)$$

have the same orthogonal sphere, the radius of which equals $(c_1{}^2/4-\mathrm{R}^2)^{\frac{1}{2}}$. Every pair of spheres also has the same mutual coefficient as two spheres each of radius R at a distance c_1 apart. The equations (20) and (21) enable us to calculate the mutual coefficient between any two spheres by means of the tables, or by the very simple formulæ given below for two equal spheres.

It is also easy to show that the two spheres whose constants are given by

$$\left.\begin{aligned} a&=\frac{c_1}{2}\sqrt{n^2-n}+\left\{\frac{c_1{}^2}{4}(n^2-n)+n\mathrm{R}^2\right\}^{\frac{1}{2}} \\ b&=-\frac{c_1}{2}\sqrt{n^2-n}+\left\{\frac{c_1{}^2}{4}(n^2-n)+n\mathrm{R}^2\right\}^{\frac{1}{2}} \\ \text{and} \qquad c&=nc_1 \end{aligned}\right\}, \quad . \quad (22)$$

where n is any number not less than unity, have the same mutual coefficient as two equal spheres of radius R at a distance c_1 apart.

The Self-coefficients for Equal Spheres can be computed from the Tables for the Mutual Coefficients.

It is easy to show from Maxwell's formulæ that

$$\frac{k_{1,1}}{\mathrm{R}}=\frac{k_{1,2}}{\mathrm{R}}-\frac{k'_{1,2}}{\mathrm{R}'}\left\{\frac{c}{\mathrm{R}}+2\right\}^{\frac{1}{2}} \quad . \quad . \quad . \quad (23)$$

where $c'/\mathrm{R}'=\{c/\mathrm{R}+2\}^{\frac{1}{2}}$. $k'_{1,2}$ is the mutual coefficient for two equal spheres of radius R' at a distance c' apart. Since c'/R' is known, $k'_{1,2}/\mathrm{R}'$ can be written down from the tables and hence, by (23), $k_{1,1}$ can be found.

Approximate Formulæ for the Capacity Coefficients between Equal Spheres.

When the distance between the spheres is very small the author's formulæ (*l.c. ante*) are the only ones that can be used. For distances between them less than their diameter Kelvin's* and the author's† tables can be used. In the latter tables a small error affects the computed values of the coefficients for the case of $x/\mathrm{R}=0{\cdot}45$. In this case the true values are

$$k_{1,1}/\mathrm{R}=1{\cdot}27173, \qquad -k_{1,2}/\mathrm{R}=0{\cdot}54728,$$

$$-\tfrac{1}{2}\frac{\partial \mathrm{K}_{1,1}}{\partial x}=0{\cdot}19961, \qquad \tfrac{1}{2}\frac{\partial \mathrm{K}_{1,2}}{\partial x}=0{\cdot}23216.$$

For values of c greater than 3R we can use the simple formulæ

$$\frac{k_{1,1}}{\mathrm{R}}=1+\frac{1}{y^2}+\frac{2y^2+1}{(y^3+y)^2-(2y^2+1)^2}-\frac{1}{y^8} \quad . \quad . \quad . \quad (24)$$

and

$$-\frac{k_{1,2}}{\mathrm{R}}=\frac{1}{y}+\frac{y^3+y}{(y^3+y)^2-(2y^2+1)^2}, \quad . \quad . \quad . \quad . \quad . \quad (25)$$

where $y=c/\mathrm{R}$. When $c/\mathrm{R}=3$, the maximum inaccuracy of these formulæ is 1 in 10,000. For greater values of c they are much more accurate.

The Capacity between unequal Spherical Electrodes.

It is easy to show that

$$\frac{q}{v_1-v_2}=\frac{k_{1,1}\,k_{2,2}-k^2_{1,2}}{k_{1,1}+k_{2,2}+2k_{1,2}}. \quad . \quad . \quad . \quad . \quad (26)$$

when the charges on the spheres are q and $-q$ respectively and all neighbouring conductors are at zero potential. In this case the ratio of the charge to the difference of potential is constant and as it is convenient to give it a name the author ‡ calls it the capacity between the conductors, when all neighbouring conductors are earthed. By means of the formulæ given above it can be readily computed when there are no other conductors near the spheres. When the spheres are equal, it simplifies to $(k_{1,1}-k_{1,2})/2$. For instance, when $a=b=72$ cms. and $c=180$ cms., then, from the values calculated above, we find that the capacity between the spheres$=64\cdot0222$ cms.$=0\cdot00007114$ microfarad.

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* Reprint, p. 466. Second edition.

† *L.c. ante.*

‡ "Journ. of the Inst. Elec. Eng.," Vol. XXX., p. 1022, 1901.

Two spheres, the distance between which can be adjusted by means of a screw, form a very convenient adjustable standard capacity, the values of which in various positions can be written down almost at once to any required accuracy.

The Potentials of the Spheres for Given Charges.

The values of v_1 and v_2 can be easily found from formulæ (1). In the case when the charges are equal and opposite,

$$-\frac{v_1}{v_2}=\frac{k_{2,2}+k_{1,2}}{k_{1,1}+k_{1,2}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

When the distance between the spheres is infinite

$$-\frac{v_1}{v_2}=\frac{b}{a},$$

and when they are very close together

$$-\frac{v_1}{v_2}=\frac{\psi\{a/(a+b)\}+\gamma}{\psi\{b/(a+b)\}+\gamma}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

For example, when $a=1$ and $b=3$, then at infinity $-v_1/v_2=3$, and when they are almost touching

$$-\frac{v_1}{v_2}=\frac{\psi(1/4)+\gamma}{\psi(3/4)+\gamma}=\frac{6\log 2+\pi}{6\log 2-\pi}=7\cdot178.$$

When the spheres are at the same potential

$$\frac{q_1}{q_2}=\frac{k_{1,1}+k_{1,2}}{k_{2,2}+k_{1,2}},$$

and hence when they are close together

$$\frac{q_1}{q_2}=\frac{\psi\{b/(a+b)\}+\gamma}{\psi\{a/(a+b)\}+\gamma}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

When the spheres whose radii are 1 and 9 respectively, for instance, are at a great distance apart and are at the same potential $q_1/q_2=0\cdot1111$, but when they are touching $q_1/q_2=0\cdot07667$.

Finally, when the charges on the spheres are equal

$$\frac{v_1}{v_2}=\frac{k_{2,2}-k_{1,2}}{k_{1,1}-k_{1,2}}.$$

In this case, when the spheres are a great distance apart $v_1/v_2=b/a$, but when they are very close together $v_1/v_2=1$.

The Capacity Currents to Spherical Electrodes.

If i_1 and i_2 denote the instantaneous values of the currents in the leads to the electrodes, we have

$$\left.\begin{aligned} i_1 &= \frac{\partial q_1}{\partial t} = k_{1,1}\frac{\partial v_1}{\partial t} + k_{1,2}\frac{\partial v_2}{\partial t} \\ \text{and} \qquad i_2 &= \frac{\partial q_2}{\partial t} = {}_{2,2}\frac{\partial v_2}{\partial t} + k_{1,2}\frac{\partial v_1}{\partial t} \end{aligned}\right\} \quad . \quad . \quad . \quad . \qquad (30)$$

Hence, when the laws according to which the potentials of the terminals vary are known the capacity currents can be found. In practice we generally have either $q_1 = -q_2$ or $v_2 = 0$ at every instant. In the former case,

$$i_1 = -i_2 = \frac{{}_{1,1}k_{2,2} - k^2_{1,2}}{k_{1,1} + k_{2,2} + 2k_{1,2}} \frac{\partial}{\partial t}(v_1 - v_2) \quad . \quad . \qquad (31)$$

and, in the latter,

$$i_1 = (k_{1,1}/k_{1,2})i_2 = k_{1,1}\frac{\partial v_1}{\partial t}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (32)$$

ABSTRACT.

Formulæ have been given by Poisson, Kelvin, Kirchhoff, Clerk Maxwell and others for the capacity coefficients of spherical conductors at appreciable distances apart. With the exception of Kirchhoff's formulæ, these formulæ are rarely, if ever, used in practice. The author shows how the remainder after computing a few terms of Kirchhoff's series formulæ can be easily found. The range of their usefulness is therefore considerably extended. This extension makes it possible to simplify very appreciably the author's formulæ for the case, when the spheres are close together. Hence he shows that the numerical computation of the capacity coefficients can be easily made in all cases. Applications showing how the potentials, the capacities and the capacity currents of spherical electrodes can be calculated are given. A simple method of making a variable air condenser whose capacity in every position can be easily calculated to any desired accuracy is also given.

DISCUSSION.

Dr. ECCLES pointed out that the readjustment of the terms of a series was not justifiable unless the series was uniformly convergent for the values for which it was to be used. Regarding the proposed standard of capacity made of two opposed spheres, if the capacity be made large, and therefore the distance between the spheres small, errors in the distance become very important. This is in somewhat unfavourable contrats with the concentric cylinder type of condenser, where small errors in setting the axes of the cylinders are not harmful.

The AUTHOR pointed out that the series in question was uniformly and absolutely convergent. As a screw is used to adjust the distance between the spheres, the percentage errors need not be greater than the corresponding errors when measuring lengths with a screw gauge. The great advantage of the condenser formed by two spheres is that its capacity can be found at once with high accuracy in all positions. The capacity of a cylindrical condenser can only be accurately calculated when guard cylinders are used. In this case its capacity cannot be varied.

XXXV. *On the Effect of a Narrow Saw-cut in the Edge of a Conducting Strip on the Potential and Stream Lines in the Strip and on the Resistance of the Strip.* By CHARLES H. LEES, *D.Sc., F.R.S., Professor of Physics in the East London College, University of London.*

RECEIVED MAY 25, 1911.

AT a meeting of the Society some time ago I was asked what would be the effect of a narrow saw-cut in the edge of a standard low-resistance strip, and I have recently put the result of the calculation of the effect into a form suitable for practical purposes.

The conducting strip is taken of breadth unity, of thickness and material such that when unit current flows along it a fall of potential of one unit per unit of length is produced. If the strip is of great length and the saw-cut near the centre of one edge,

$$\frac{dz}{d\zeta}=\mathrm{B}\zeta(\zeta^2-1)^{-1}(\zeta^2-a^2)^{-\frac{1}{2}},$$

where B is a constant, is the Schwarzian equation* for transforming the contour of the strip in the z plane, with the outer end of the cut as origin of co-ordinates, into the axis of the real variable in the ζ plane, the inner end of the cut becoming the origin of co-ordinates, the outer ends the points $\pm a$, where $a<1$, and the ends of the strip the points ± 1 on the real axis in the ζ plane.

Hence
$$z=\mathrm{B}\int\frac{\zeta d\zeta}{(\zeta^2-1)\sqrt{\zeta^2-a^2}}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Putting $\sqrt{\zeta^2-a^2}=\sqrt{1-a^2}\tanh\left(\theta\pm i\frac{\pi}{2}\right)$, the $i\frac{\pi}{2}$ being suppressed if $\zeta<1$, we have

$$(\zeta^2-a^2)^{-\frac{1}{2}}\zeta \, . \, d\zeta=\sqrt{1-a^2}\,\mathrm{sech}^2\left(\theta\pm i\frac{\pi}{2}\right)d\theta,$$

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* *See* Werke or Crelle, 70, p. 105 ; Sir J. J. Thomson "Recent Researches," Chap. III.

and the integral reduces to

$$z=-\frac{B}{\sqrt{1-a^2}}\int d\theta=-\frac{B}{\sqrt{1-a^2}}\,.\,\theta+\text{constant},$$

that is, $$\sqrt{\frac{\zeta^2-a^2}{1-a^2}}=\tanh\left(-\frac{z\sqrt{1-a^2}}{B}\pm i\frac{\pi}{2}\right),$$

the constant being put$=0$.

Since the breadth of the strip is to be unity $-\frac{\sqrt{1-a^2}}{B}=\frac{\pi}{2}$ and we have finally

$$\sqrt{\frac{\zeta^2-a^2}{1-a^2}}=\tanh\frac{\pi}{2}(z\pm i), \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the i being suppressed if $\zeta<1$.

The depth c of the cut is given by $i\sqrt{\frac{a^2}{1-a^2}}=\tanh\frac{\pi}{2}ic$,

i.e., $$\sqrt{\frac{a^2}{1-a^2}}=\tan\frac{\pi}{2}c, \quad \text{or} \quad a=\sin\frac{\pi}{2}c.$$

If the potential function $w=u+iv$ for any distribution of sources and sinks in the upper half of the ζ plane is known as a function of ζ, it is only necessary to eliminate ζ from the function by means of equation (2) to have the potential function for the corresponding distribution of sources and sinks in the z plane.

For the present purpose we take a source of strength unity at the point $+1$ sending current into the upper half of the ζ plane and a sink of the same strength at the point -1 absorbing this current. The potential function w is then given by the equation

$$w=\frac{1}{\pi}\log\frac{1-\zeta}{1+\zeta}$$

or $\zeta=\tanh\frac{\pi}{2}(w\pm i)$, the i being suppressed if $\zeta<1$. . (3)

Eliminating ζ from the equations (2) and (3) we have

$$\sqrt{1-a^2}\cosh\frac{\pi}{2}(w\pm i)=\cosh\frac{\pi}{2}(z\pm i),$$

that is, $$\cosh\frac{\pi}{2}(w\pm i)=\cosh\frac{\pi}{2}(z\pm i)/\cos\frac{\pi}{2}c,$$

for the potential function w of the strip.

Since w and z will be taken to refer to the same part of the plane we may write the result

$$\cosh \frac{\pi}{2}w = \cosh \frac{\pi}{2}z / \cos \frac{\pi}{2}c. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

When the unreal part v of w is zero we have either y_0 the unreal part of $z=0$, and $\cosh \frac{\pi}{2}u_0 = \cosh \frac{\pi}{2}x / \cos \frac{\pi}{2}c$, connecting the real parts u_0 and x_0, or $x_0=0$ and $\cosh \frac{\pi}{2}u_0 = \cos \frac{\pi}{2}y_0 / \cos \frac{\pi}{2}c$, connecting the real part of w with the unreal part of z.

From either equation it follows that the potential at the outer end of the cut$=\frac{2}{\pi}\mathit{arg} \cosh \sec \frac{\pi}{2}c$.

When the unreal part v of w is $+1$, y is $+1$, and we have

$$\cosh \frac{\pi}{2}(u_1+i) = \cosh \frac{\pi}{2}(x_1+i) / \cos \frac{\pi}{2}c,$$

or

$$\sinh \frac{\pi}{2}u_1 = \sinh \frac{\pi}{2}x_1 / \cos \frac{\pi}{2}c,$$

connecting the real parts of w and z.

At a considerable distance, x_0, from the cut along the cut edge we have

$$\sqrt{1-a^2}\,e^{\frac{\pi}{2}u_0} = e^{\frac{\pi}{2}x_0} \quad \text{or} \quad u_0 = x_0 + \frac{2}{\pi}\log \sec \frac{\pi}{2}c$$

as a first approximation and

$$\sqrt{1-a^2}\,e^{\frac{\pi}{2}u_0}(1+e^{-\pi u_0}) = e^{\frac{\pi}{2}x_0}(1+e^{-\pi x_0}),$$

or

$$u_0 = x_0 + \frac{2}{\pi}\left\{\log_e \sec \frac{\pi}{2}c + \sin^2 \frac{\pi}{2}c \,.\, e^{-\pi x_0}\right\}. \quad . \quad . \quad (5)$$

as a second.

The potential difference between a point on the cut edge some distance x_0 from the cut and a point at the bottom of a cut of depth c times the breadth of the strip, is therefore the same as if x_0 were increased by $\frac{2}{\pi}\left\{\log_e \sec \frac{\pi}{2}c + \sin^2 \frac{\pi}{2}c \,.\, e^{-\pi x_0}\right\}$ if the strip is of unit breadth and by b times this expression if the strip is of breadth b.

Similarly at a point on the uncut edge some distance, x_1 from the line of the cut, we have as a first approximation

$$u_1=x_1+\frac{2}{\pi}\log_e \sec \frac{\pi}{2}c,$$

and as a second

$$u_1=x_1+\frac{2}{\pi}\left\{\log_e \sec \frac{\pi}{2}c-\sin^2 \frac{\pi}{2}c \,.\, e^{-\pi x_1}\right\}. \qquad (6)$$

The potential difference between a point on the uncut edge some distance x_1 from the line of the cut and the point on the uncut edge opposite to the cut is therefore the same as if the distance to x_1 were increased by $\frac{2}{\pi}\left\{\log_e \sec \frac{\pi}{2}c-\sin^2 \frac{\pi}{2}c \,.\, e^{-\pi x_1}\right\}$ if the strip is of unit breadth and by b times this expression if the strip is of breadth b.

The mean of the potential differences between the line of the cut and points on the two edges at equal distances from that line is the same as if the distance of the points from the line of the cut were increased by $\frac{2}{\pi}\log_e \sec \frac{\pi}{2}c$ if the breadth of the strip is unity and by b times this expression is the breadth of the strip is b.

The equations of the equipotential lines and stream lines are determined from equation (4) by expanding and equating separately real and unreal parts. We thus get

$$\left.\begin{aligned} &\cosh \frac{\pi}{2}u \,.\, \cos \frac{\pi}{2}v=\cosh \frac{\pi}{2}x \,.\, \cos \frac{\pi}{2}y/\cos \frac{\pi}{2}c \\ \text{and}\quad &\sinh \frac{\pi}{2}u \,.\, \sin \frac{\pi}{2}v=\sinh \frac{\pi}{2}x \,.\, \sin \frac{\pi}{2}y/\cos \frac{\pi}{2}c \end{aligned}\right\}.$$

Eliminating u and v respectively we have

$$2\cos^2\frac{\pi}{2}c=\frac{(\cosh \pi x+1)(1+\cos \pi y)}{\cosh \pi u+1}+\frac{(\cosh \pi x-1)(1-\cos \pi y)}{\cosh \pi x-1} \qquad (7)$$

for the equipotential, and

$$2\cos^2\frac{\pi}{2}c=\frac{(\cosh \pi x+1)(1+\cos \pi y)}{\cos \pi v+1}-\frac{(\cosh \pi x-1)(1-\cos \pi y)}{1-\cos \pi v} \qquad (8)$$

for the stream lines.

For numerical calculation of the lines it is better to get equations in x and y separately as follows :—

$$\left.\begin{aligned} 2\sec^2\frac{\pi}{2}c &= \frac{(\cosh \pi u+1)(1+\cos \pi v)}{\cosh \pi x+1}+\frac{(\cosh \pi u-1)(1-\cos \pi v)}{\cosh \pi x-1} \\ &\text{and} \\ 2\sec^2\frac{\pi}{2}c &= \frac{(\cosh \pi u+1)(1+\cos \pi v)}{1+\cos \pi y}-\frac{(\cosh \pi u-1)(1-\cos \pi v)}{1-\cos \pi y} \end{aligned}\right\} \quad (9)$$

By the aid of these equations the equipotential and stream lines for the case in which the cut extends half-way across the strip, that is, $c=\frac{1}{2}$, have been calculated and are shown below along with the equipotential lines found experimentally for a long strip of platinoid.*

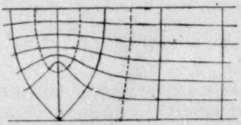

FIG. 1.—EQUIPOTENTIAL AND STREAM LINES IN STRIP WITH CUT HALF ACROSS IT AS CALCULATED.

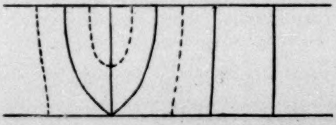

FIG. 2.—EQUIPOTENTIAL LINES OBSERVED IN LONG PLATINOID STRIP.

It will be noticed that the equipotential lines have not become perpendicular to the axis of the strip at a distance from the slit equal to two or three breadths.

In order to illustrate the distortion of the equipotential lines in the neighbourhood of the cut and to give a more definite idea as to the distance from the slit to which it is necessary to go before the lines become normal, the following table has been calculated.

The width of the strip is taken as unity and a current is supposed to pass along it which produces in the uncut strip a fall of potential of unity per unit length. The depth of the cut is c.

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* For the numerical calculation of these curves and for the experimental verification I have to thank Mr. T. E. G. McCathie, B.Sc.

Table of Positions of Points at Given Potentials on Edges of Strip of Breadth Unity.

Potential.	$c=\frac{1}{4}$.		$c=\frac{1}{2}$.		$c=\frac{3}{4}$.	
	Distances.		Distances.		Distances.	
	Uncut.	Cut.	Uncut.	Cut.	Uncut.	Cut.
0	0	0	0	0	0	0
$1 \cdot \frac{2}{\pi}$	$0{\cdot}941\frac{2}{\pi}$	$0{\cdot}893\frac{2}{\pi}$	$0{\cdot}758\frac{2}{\pi}$	$0{\cdot}424\frac{2}{\pi}$	$0{\cdot}436\frac{2}{\pi}$	0
$2 \cdot \frac{2}{\pi}$	1·924 ,,	1·918 ,,	1·671 ,,	1·634 ,,	1·131 ,,	$0{\cdot}906\frac{2}{\pi}$
$3 \cdot \frac{2}{\pi}$	2·921 ,,	2·920 ,,	2·656 ,,	2·651 ,,	2·054 ,,	2·025 ,,
$4 \cdot \frac{2}{\pi}$	3·921 ,,	3·921 ,,	3·654 ,,	3·653 ,,	3·041 ,,	3·037 ,,
$5 \cdot \frac{2}{\pi}$	4·921 ,,	4·921 ,,	4·653 ,,	4·653 ,,	4·040 ,,	4·039 ,,
$6 \cdot \frac{2}{\pi}$	5·921 ,,	5·921 ,,	5·653 ,,	5·653 ,,	5·039 ,,	5·039 ,,

From this table it will be seen that in the case of the deep cut $c=\frac{3}{4}$ it is necessary to go to a distance from the slit equal to three times the breadth of the strip before the equipotential lines become practically parallel and at unit distance apart.

Since the effect of a fine saw-cut in and perpendicular to the edge of a long resistance strip and of depth c times the breadth of the strip, is equivalent to an increase of length of the strip equal to $\frac{4}{\pi}\log_e \sec\frac{\pi}{2}c$ breadths, the depth of cut necessary to raise the resistance of the strip by any given amount may be calculated.

ABSTRACT.

By means of the Schwarzian transformation it is shown that if a fine saw-cut is made in the edge of a long conducting strip, perpendicular to the edge, the resistance of the strip is increased by an amount equivalent to that of a length of strip equal to $\frac{4}{\pi}\log_e \sec.\frac{\pi}{2}c.$ breadths, where c is the quotient of the depth of the cut by the breadth of the strip. Diagrams of the stream and potential lines near the cut are given.

XXXVI. *The Probability Distribution of the Time Intervals of α Particles with Application to the Number of α Particles emitted by Uranium. By* E. Marsden, *M.Sc., and* T. Barratt, *B.Sc., East London College, University of London.*

Received July 11, 1911.

In counting the α particles from radio-active substances by the scintillation method the great preponderance of short intervals is very noticeable, especially when the scintillations are appearing at a slow rate. In fact this preponderance leads one at first sight to consider the α particles as coming in groups and not distributed according to a simple probability law. Rutherford and Geiger* have recently examined the emission of α particles from polonium from this point of view. In their experiments scintillations produced on a definite area of a zinc-sulphide screen placed in an exhausted vessel at a distance from the polonium source of α particles, were recorded on a travelling chronograph tape and analysed according to the numbers occurring in successive equal time intervals, generally large compared with the average time interval of the scintillations. The results were found to be in good agreement with Bateman's† theoretical formula that $x^n e^{-x}/n!$ is the probability of n scintillations occurring in a given interval where x is the true average number for the interval. However, were the particles given off in equal groups and the groups distributed in time according to probability it is conceivable that the above formula would still hold. It would, therefore, seem preferable in many respects to test the application of the probability laws to actual time intervals between successive α particles and to count the whole number from a given source instead of only those emitted within a relatively small solid angle.

The problem of the calculation of the distribution of time intervals can be approached in the following way. Let the average time interval between successive α particles falling on a zinc-sulphide screen from a radio-active source be $1/\mu$, and let us assume that the time under consideration is small compared with the time period of the radio-active substance. Assuming that at time 0 the observer sees a scintillation, let us find the probability that a time interval, t, elapses without the occurrence of a second scintillation but that within a further very small interval, δt, a scintillation occurs. This problem corresponds exactly to the method employed in practice. The probability that no scintillation occurs in the time interval t is $e^{-\mu t}$.[*] The probability that a scintillation occurs in the time interval from t to $t+\delta t$ is independent of t and is $\mu\delta t$. As these events are independent of one another the probability of an interval from t to $t+\delta t$ is the product of their separate probabilities and is therefore $\mu e^{-\mu t}\delta t$; or in a large number of N intervals the probable number of intervals larger than t and smaller than $t+\delta t$ is $N\mu e^{-\mu t}\delta t$.

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* Rutherford and Geiger, "Phil. Mag." XX., p. 698, Oct., 1910.

† *Loc. cit.* Note by H. Bateman.

This result is at first sight somewhat surprising,[†] for it indicates that whatever the value of μ small intervals are more probable than large ones, whereas one would at first sight expect that the intervals would be distributed according to a law somewhat similar to that of Maxwell for the distribution of the velocities of the molecules of a gas.

The above formula has been applied to several sets of observations made by us on the α particles from polonium and uranium and found to agree well with experiment. The case of uranium is particularly interesting, for Boltwood[‡] and Geiger and Rutherford[§] have shown that uranium emits two α particles. If these α particles are given off together or by successive α ray products, the second having a period less than a few seconds, then in observing scintillations caused by α particles from uranium placed in contact with a zinc-sulphide screen one would expect a larger number of short intervals than according to the simple theory above. The experimental arrangement used to test this point is shown in Fig. 1 and is the same as that of Geiger and Marsden[‖] in their experiments on the α particles from actinium and thorium emanations. S_1 and S_2 are two screens of zinc-sulphide coated on glass with the zinc-sulphide coatings facing each other and separated by sheets of aluminium foil. One of these sheets was coated with a very fine layer of uranium-oxide produced by pouring on it uranium-oxide suspended in alcohol and allowing the alcohol to evaporate. Two microscopes, M_1 and M_2, were focussed on exactly opposite areas of the two screens and the scintillations observed were recorded by two separate observers on the same travelling tape of a Morse inker by means of two needle points arranged to puncture the tape by arrangements of levers. Ink marks were also made on the tape at second intervals by means of a circuit containing an electromagnet and a pendulum contact. Just sufficient sheets of thin aluminium foil were used that no scintillations on one screen were visible in the microscope focussed on the other, more than one being necessary on account of the difficulty of keeping it unpunctured by the crystals of the screen on subsequently pressing them together. When the

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[*] H. Bateman, *loc. cit.* It is not at first sight clear that we are justified in applying this formula to a time interval commencing with a scintillation, but the time of occurrence of the scintillation is really arbitrary as referred to the times of previous scintillations. The result may also be obtained as follows: The probability that a time interval that does not contain a scintillation is $e^{-\mu t}$, and this is, therefore, the probability that an interval without scintillation is greater than t, or in N such intervals the number greater than t is $Ne^{-\mu t}$. Similarly, the number of intervals which are greater than $t+\delta t$ is $Ne^{-\mu(t+\delta t)}$. The difference gives us the number of intervals between successive scintillations greater than t but smaller than $t+\delta t$, and is $N\mu e^{-\mu t}\delta t$.

[†] *Cf.* Rutherford and Geiger, Roy. Soc. "Proc.," A. LXXI., p. 141 (1908).

[‡] Boltwood, "Amer. Journ. Sci.," Vol. XXV., p. 270 (1908).

[§] Geiger and Rutherford, "Phil. Mag.," XX., p. 692, 1910.

[‖] Geiger and Marsden, "Phys. Zeit.," XII., p. 7, 1910.

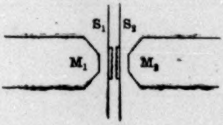

FIG. 1.

screens were placed together in this way they were found to be about $\frac{1}{3}$ mm. apart, while the diameters of the fields of the two similar microscopes were about 2·3 mm., so that practically the whole of the particles given off by the uranium between the screens would impinge on either one or the other of them. In making the observations the tape was started and scintillations recorded by both observers for a period of from two to five minutes. The tape was then stopped and after the eyes had been rested another set of observations was made, and so on, until a sufficient number of scintillations had been observed. The records on the tape were then examined and a curve drawn giving the number of occurrences of different intervals and this curve compared with that obtained from the formula given above. Thus in the particular case shown in Fig. 2.

Total time=2,988·5 seconds.

Number of intervals=(taking the observations on both screens) 319.

Average interval=9·37 seconds. μ=0·1068 (approx.).

The probability of an interval from 0 to 1 second is, to the approximation sufficient for the present experiment, the value of $N\mu e^{-\mu t}$ for $t=0{\cdot}5=319\times 0{\cdot}1068\ e^{-0\ 0}\ =32{\cdot}2$.

The actual number of such observations was 36.

In this way the probability curve shown in Fig. 2 has been drawn and it will be seen that the actual values observed, which are indicated by the points, agree with it to an extent within the limits of the probability error. The tape was also examined by the method of Rutherford and Geiger, the number of scintillations in successive intervals of 16 seconds being measured. The number of occurrences of each number are given in column I. of the following table, while in column II. are the numbers calculated from Bateman's formula.

No. of particles in interval of 16 seconds.	No. of groups.	
	I. Experimental.	II. Calculated.
0	35	34
1	56	58
2	49	49·5
3	32	28
4	9	12
5	5	4·1
6	1	1·15

The agreement is good, and considering the small number of observations it is probably fortuitous. Good agreement between experiment and theory was also found in other cases in which the amount of uranium was varied so as to give different rates of emission of α particles, while experiments in which a layer of polonium was used also showed excellent agreement with theory. The polonium was obtained by evaporating on an aluminium foil a solution of the active deposit on the walls of an old tube which had contained radium emanation. The emission of α particles was also examined by the foregoing methods on two separate screens placed at various distances up to 83 cm. from sources of polonium. Agreement between experiment and theory was in all cases satisfactory.

Through the kindness of Prof. Rutherford and Dr. Geiger, we have been allowed to examine some of the records of the scintillations made in their experiments on the "Probability Variations in the Distribution of α Particles." These also showed excellent agreement with the above theory.

The theory of these observations is independent of whether the screens are continuous or whether scintillations are only produced by a fraction of the α particles. However (with the screens used) it is unlikely that more than 10 per cent. of the α particles failed to produce scintillations. The chief source of error might be the missing of scintillations through temporary fatigue of the eyes, for in the case of uranium the range of the particles is only 2·7 cm. and their velocity consequently small, this being further cut down by the aluminium foils. Using the low magnification necessary to obtain a large field and, with the particular objectives used, the consequent small solid angle of the light from the scintillations, they were much fainter than in usual scintillation experiments. The missing of scintillations, however, is partly avoided by the observations of one observer forming a check on those of the other. To test this point further and to verify the experimental arrangement the observations of Geiger and Marsden* on the double scintillations from actinium emanation were repeated. The emana-

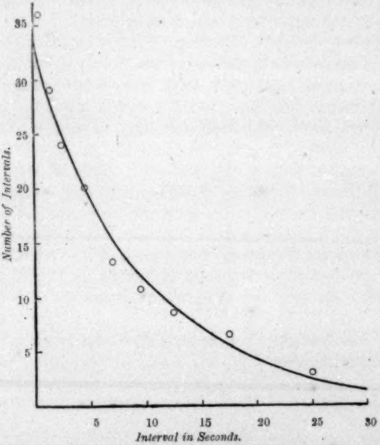

FIG. 2.

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* *Loc. cit.*

tion was allowed to diffuse up between the two screens and the same numbers of connected scintillations were observed as in their experiments. Thus, with the emanation and active deposit in equilibrium nearly 50 per cent. of the scintillations appeared simultaneously on either the same or both screens, and a simple calculation showed that with the arrangement used only a few of the *a* particles could have been missed. As a further support of the theory it was found that when the doubles with actinium emanation were neglected and counted as single scintillations the distribution of the remaining time intervals was in agreement with theory.

We thus see that the variation of the intervals between the emission of successive *a* particles from both polonium and uranium shows good agreement with simple probability theory, and we therefore conclude that the two *a* particles from uranium are not given off simultaneously. Further, if they are given off by successive *a*-ray products the period of the second must be greater than a few seconds. This is also unlikely because of the small ranges of both sets of *a* particles.*

Further experiments are in progress to apply the foregoing method to the *a* particles from thorium B and thorium C and also to investigate the possible existence of a short-life product in the active deposit of actinium indicated by the 10 per cent. of connected particles found in the experiments of Geiger and Marsden.

We are indebted to Prof. Lees, F.R.S., for his kind interest in these experiments.

ADDENDUM.

[JULY 18, 1911.]

Prof. Rutherford and Dr. Geiger have very kindly allowed us to publish the figures relating to observations on some of the records of the scintillations obtained in their experiments. The time intervals between successive scintillations were deduced from the measured distance between the records on the tapes, allowance being made for the varying velocity of the tape by reference to the time marks. The time marks were made at 30 seconds intervals and the average distance between them was about 7 cm.

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* A. Foch, "Le Radium," VIII., p. 101, 1911.

The results are given in the following table. Column II. gives the experimentally determined number of intervals of duration between the limits given in Column I. The theoretical numbers in Column III. were calculated as follows:—

Total number of intervals .... = 7,564
Total time .... =14,598 secs.
Average interval .... = 1·930 secs.

$$\mu=0{\cdot}5181.$$

Number of intervals of duration greater than t_1 and less than

$$t_2=\int_{t_1}^{t_2} \mathrm{N}\mu e^{-\mu t}dt=\mathrm{N}(e^{-\mu t_1}-e^{-\mu t_2})=7{,}564(e^{-0{\cdot}5181t_1}-e^{0{\cdot}5181t_2}).$$

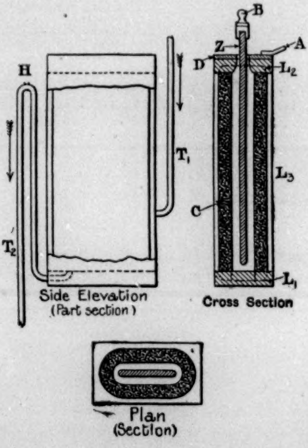
Duration of interval between successive scintillations. Seconds.	Number of intervals.	
	Experimental.	Calculated.
0·0 to 1·0	3,106	3,059
1·0 to 2·0	1,763	1,822
2·0 to 3·0	1,115	1,085
3·0 to 4·0	658	646
4·0 to 5·0	389	385
5·0 to 6·0	206	229
6·0 to 7·0	130	136
7·0 to 8·0	86	81
8·0 to 9·0	42	48
9·0 to infinity	68	71

The agreement between the experimental and theoretical values is surprisingly good and is probably well within the probability error.

XXXVII. *On the Benkö Primary Battery.* *By* W. R. COOPER, *M.A., B.Sc.*

JUNE 30, 1911.

THE Benkö primary battery, due to Mr. Stephan Benkö, of Buda-Pest, depends essentially upon the use of a porous carbon electrode through which the electrolyte flows. The construction is shown diagrammatically in the adjoining figure. Here the carbon, which may be described as a flattened cylinder open at both ends, is indicated at C. This is closed at the bottom by a lead cap, L_1, and the top is finished off by a lead ring L_2, these being put on under pressure so that intimate contact is made with the carbon. A lead shell, L_3, is then fitted round the carbon, leaving a small space between the carbon and the shell, and is jointed to the lead cap and lead ring, thus forming a chamber round the carbon. One terminal is fitted to the lead L_2 and the other is formed by the zinc plate Z, which is held in an ebonite frame. The electrolyte is introduced under a suitable head into the lead shell by a tube, T_1; it flows through the carbon to the zinc and



thence to waste through an inverted U tube T_2, so that the electrolyte is maintained at a proper level. Syphoning is prevented by a small hole at H.

The electrolyte (a sodium bichromate solution) is supplied at such a head as to give a flow of ½ litre, or more, per hour per cell, depending upon the current required. The E.M.F. is 2 volts.

Polarisation is thus reduced to a minimum and remains constant for a given current, provided the cell is not overloaded. The size of cell referred to is 9¼ in. high overall, 6¼ in. long and 1¾ in. wide, weighs about 10 lb. excluding the tank for electrolyte, and will give a steady current (depending upon the strength of the electrolyte and rate of flow) up to, say, 25 amperes at 1·5 volts. The internal resistance is below 0·01 ohm. There is no "mess" and no difficulty in keeping in order. The cell is, therefore, very suitable for laboratory use, or work where the cost is not of prime importance. Various types are being made according to the work required, and include cells which depend for their action upon diffusion as distinct from a steady flow.

DISCUSSION.

Mr. G. L. **Addenbrooke** asked whether the contact between the lead and the carbon remained satisfactory over a long time.

The **Author** replied in the affirmative so far as his experience (extending over 15 months) had gone.

The **President** inquired as to the maximum number of watts that could be taken from the battery continuously, and whether, weight for weight, more power could be taken from it than from accumulators.

The **Author** replied to the first part of the question about 25 to 40 watts, depending on the solution, and stated that, excluding the reservoir, 2·4 to 4 watts per pound could be taken from the battery as compared with ½ watt (8-hour rate) up to 2½ watts (automobile cells at 5-hour rate) for accumulators.

Replying to Dr. **Willows** the author said that the zinc could be taken out, or the electrolyte could be run out when the cell was not in use.

Mr. W. **Duddell** asked how the battery would compare with accumulators for giving a steady current of the order of 5 amperes to 1 part in 5,000 or so. Accumulators could only be relied on to keep steady to about 1 part in 1,000.

The **Author** said he could not reply definitely, but he would expect a better result from the Benkö battery bulk for bulk. It would be necessary to maintain a constant temperature.

Mr. **Frazer** inquired how long the diffusion cell lasted before it became an ordinary cell.

The **Author** replied that the E.M.F. began to fall after about 10 hours in use, and the diffusion would probably be complete for practical purposes in about five days in the small cells if not in use.

Mr. S. W. **Melsom** asked whether the electrochemical efficiency of the zinc was the same as in an ordinary primary cell.

The **Author** stated that it was about the same.

INDEX.

A.

PAGE

Afterglow of electric discharge 66

Airey, J. R., on the roots of Neumann and Bessel functions 219

Airey, J. R., on the vibrations of circular plates and their relations to Bessel functions 225

Alpha particles, on the probability distribution of the time intervals of 367

Analysis, harmonic, on a new method of, by selected ordinates 334

Anderson, A., and Bowen, J. G., on the measurement of contact differences of potential 346

Anschütz gyro-compass 201

B.

Barratt, T., and Marsden, E., on the probability distribution of the true intervals of α particles, with application to the number of α particles emitted by uranium 367

Benkö primary battery, the 374

Bessel functions, on their connection with the vibrations of circular plates 225

Bessel and Neumann functions, on the roots of 219

Boundary problems, approximate solution of, by surface integration 75

Bowen, J. G., and Anderson, A., on the measurement of contact differences of potential 346

Budd, L. C., and Wilson, E., on previous magnetic history affected by temperature 253

C.

Callendar, H. L., on the caloric theory of heat and Carnot's principle 153

Callendar, H. L., on the radio balance: a thermoelectric balance for the absolute measurement of radiation 1

Caloric theory of heat and Carnot's principle 153

Capacity coefficients of spherical electrodes 352

Capillary tube, on the movement of a coloured index along 190

Carnot's principle and the caloric theory of heat 153

Chree, C., on the supposed propagation of equatorial magnetic disturbances 49

Circuit, coupled, on oscillatory currents in 237

Circular and hyperbolic functions, tables of, for complex values of the argument 344

Circular plates, on the vibrations of 225

Coefficients, capacity, of spherical electrodes 352

PAGE

Concave grating, on the curvature of the spectrum lines in 233
Condensers, energy losses in 117
Contact differences of potential, on the measurement of 346
Cooper, W. R., on the Benkö primary battery 374
Coupled circuits, on oscillatory currents in 237
Cusped waves of light and the theory of the rainbow 58

D.

Discharge, electric, afterglow of 66
Discharges, high tension, a new method of producing 35
Displacements, determination of small, with high power optical lever 198
Distribution, probability, of the time intervals of α particles 367
Dust striations, on the formation of by an electric spark 264
Dyke, G. B., and Fleming, J. A., on the measurement of the energy losses in condensers traversed by high-frequency oscillations ... 117
Dyke, G. B., and Fleming, J. A., on resonance curves taken with the impact and spark-ball discharger 136

E.

Eagle, A., on the curvature of the spectrum lines in a concave grating 233
Eccles, W. H., on an electrical Trevelyan rocker 204
Electric discharge, afterglow of 66
Electric stress at which ionisation begins in air 86
Electrodes, spherical, on the capacity coefficients of 352
Electroscope, on a tilted gold leaf 209
Electrostatic wattmeters, on high tension 246
Elphinstone, G. K. B., on the Anschütz gyro-compass 201
Energy losses in condensers 117
Equatorial magnetic disturbances, on the supposed propagation of 49

F.

Flames of low temperature supported by ozone 147
Fleming, J. A., and Dyke, G. B., on the measurement of energy losses in condensers traversed by high-frequency oscillations...117
Fleming, J. A., and Dyke, G. B., on resonance curves taken with impact and spark-ball discharger 136
Fleming, J. A., on the experimental measurement of the high-frequency resistance of wires 103
Flux, on the method of constant rate of charge of, for determining magnetisation curves 277
Functions, circular and hyperbolic, tables of, for complex values of the argument 344
Functions, Neumann and Bessel, on the roots of 219

G.

Grating, concave, on the curvature of the spectrum lines in 233
Griffiths, A., on the movement of a coloured index along a capillary tube 190
Gyro-compass, Anschütz 201

H.

PAGE

Harmonic analysis, on a new method of, by selected ordinates 334
High-frequency resistance of wires, measurement of...................... 103
High-tension discharges, a new method of producing 35
High-tension electrostatic wattmeters, on 246
History, on previous magnetic, as affected by temperature 253
Howe, G. W. O., on oscillatory currents in coupled circuits 237
Howe, G. W. O., on some wireless telegraphic apparatus at the City and Guilds College 301
Hyperbolic and circular functions, tables of, for complex values of the argument 344

I.

Impact discharger, resonance curves taken with 136
Index, coloured, movement of, along a capillary tube 190
Ionisation, electric stress at which it begins in air 86

J.

Johnstone, A., tables of circular and hyperbolic functions for complex values of the argument 344

K.

Kathodes, lime, on the behaviour of incandescent 257
Kaye, G. W. C., on the tilted gold leaf electroscope 209

L.

Langford, T. H., and Morris, J. T., on the method of constant rate of change of flux for determining magnetisation curves............ 277
Lees, C. H., on the effect of a narrow saw-cut in a conducting strip...
Lever, high-power optical.. 198
Lime kathodes, on the behaviour of incandescent 257
Low temperature flames supported by ozone.............................. 147
Lüders' lines in mild steel, on the 305

M.

Magnetic disturbances, equatorial, on the supposed propagation of 49
Magnetic history as affected by temperature.............................. 253
Magnetisation curves determined by the method of constant rate of change of flux 277
Marsden, E., and Barratt, T., on the probability distribution of the time intervals of α particles, with application to the number of α particles emitted by uranium.. 367

PAGE

Marsh, S., and Nottage, W. H., on the formation of dust striations by an electric spark ... 264
Mason, W., on the Lüders' lines in mild steel ... 305
Measurement of contact differences of potential, on the ... 346
Measurement of energy losses in condensers ... 117
Measurement of the high-frequency resistance of wires ... 103
Morris, J. T., and Langford, T. H., on the method of constant rate of change of flux for determining magnetisation curves ... 277
Morton, W. B., on cusped waves of light and the theory of the rainbow ... 58

N.

Neumann and Bessel functions, on the roots of ... 219
Nottage, W. H., and Marsh, S., on the formation of dust striations by an electric spark ... 264

O.

Optical lever of high power ... 198
Oscillatory currents in coupled circuits ... 237
Ozone, on low temperature flames supported by ... 147

P.

Phase difference between primary and secondary currents of a transformer ... 98
Picton, T., and Willows, R. S., on the behaviour of incandescent lime kathodes ... 257
Plates, circular, on the vibrations of ... 225
Probability distribution of the time intervals of a particles ... 367

R.

Radio-balance ... 1
Radium, applications of the thermoelectric balance to ... 1
Rainbow, theory of, and cusped waves of light ... 58
Rayner, E. H., on an optical lever of high power suitable for the determination of small thicknesses and displacements ... 198
Resistance, high frequency, of wires, measurement of ... 103
Resonance curves taken with an impact and spark-ball discharger ... 136
Richardson, L. F., on the solution of various boundary problems by surface integration ... 75
Rocker, an electrical Trevelyan ... 204
Rogers, F., behaviour of steel under combined static stress and 39 shock ... 39
Roots of Neumann and Bessel functions ... 219
Russell, A., on the capacity coefficients of spherical electrodes ... 352
Russell, A., on the electric stress at which ionisation begins in air ... 86

S.

PAGE

Saw-cut, on the effect of, in a conducting strip 361
Spectrum lines, on the curvature of, in a concave grating 233
Spherical electrodes, on the capacity coefficients of 352
Static stress and shock, behaviour of steel under 39
Steel, behaviour of, under combined static stress and shock 39
Steel, mild, on the Lüders' lines in 305
Stress, electric, at which ionisation begins in air 86
Striations, dust, on the formation of, by an electric spark 264
Strip, conducting, on the effect of a narrow saw-cut in 361
Strutt, R. J., on the afterglow of electric discharge 66
Strutt, R. J., on flames of low temperature supported by ozone 147
Surface integration, the solution of various boundary problems by 75

T.

Telegraphic apparatus, wireless, in use at the City and Guilds' College 301
Thermo-electric balance for the absolute measurement of radiation 1
Thompson, S. P., on a new method of approximate harmonic analysis by selected ordinates 334
Tilted gold leaf electroscope 209
Transformer, phase difference of primary and secondary currents in 98
Trevelyan rocker, an electrical 204
Trouton, F. T., on the phase difference of primary and secondary currents of a transformer 98

V.

Vibrations of circular plates 225

W.

Wattmeters, on high-tension electrostatic 246
Willows, R. S., and Picton, T., on the behaviour of incandescent lime kathodes 257
Wilson, E., and Budd, L. C., on previous magnetic history affected by temperatures 253
Wilson, E., on high-tension electrostatic wattmeters 246
Wilson, E., and Wilson, H. W., on a new method of producing high-tension discharges 35
Wireless telegraphic apparatus at the City and Guilds College 301

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